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THE COMPARATIVE EFFECTIVENESS OF SIX PHENOTHIAZINE COMPOUNDS, PHENOBARBITAL AND INERT PLACEBO IN THE TREATMENT OF ACUTELY ILL PATIENTS: GLOBAL MEASURES OF SEVERITY OF ILLNESS[1]

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INTRODUCTION

Psychopharmaca (21) has listed 1,067 studies of the therapeutic effects of chlorpromazine published between 1952 and 1957, of which some 70 reported research in which an attempt was made to control relevant variables. At present the number of studies of chlorpromazine has reached astronomic magnitude. From this flood of publications, Heilizer (11) selected for review those studies which met minimum criteria of scientific research design and control procedures. There were 37 such controlled studies. The discrepancy between the volume of literature and the number of controlled studies reported points up a lag between widespread hopeful curiosity about psychiatric drugs and the development of suitable techniques and procedures for evaluating their efficacy. This lag has become increasingly serious as promising results reported from the use of chlorpromazine have been followed by the introduction of additional phenothiazine derivatives with varied claims as to their special purposes and advantages.

In January, 1958, a study of the relative effectiveness of promazine, chlorpromazine, mepazine, trifluprom azine, prochlorperazine and perphenazine was initiated on the admission service of Spring Grove State Hospital, Baltimore. The general purpose of this research was a comparison of the efficacy of these phenothiazine compounds in the short-term treatment of acutely ill patients. A grant from the National Institutes of Health (U. S. Public Health Grant No. MY-2152) supported the research, which was carried out in a large-scale, controlled operation continuing over a period of approximately two years. (Since the comparative phenothiazine study reported here constituted the first phase of research carried out under this USPHS grant, it came to be known by personnel involved as MY-2152, a designation which will be frequently used below for the sake of brevity.)

At the time Project MY-2152 was initiated, comparative multiple drug studies (*i.e.*, studies involving more than four drug categories) were limited to a few investigations, none involving acutely disturbed patients. There were no reports of controlled

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multiple comparative evaluations of phenothiazine compounds in the literature. Pennington (19, 20), Bender and Nichtern (3) and Feldman (7) reported multiple drug studies in which both phenothiazine compounds and rauwolfia derivatives were involved. Moyer *et al.* (18) and Raymond *et al.* (22) conducted similar studies using populations composed of psychiatric outpatients. As Freyhan pointed out in connection with a comparative phenothiazine study which he reported in 1959 (8), drugs of different chemical families cannot be compared, *i.e.*, calibrated or placed on a continuum with respect to potency, neurologic effect, or any other property. The appearance of a number of new phenothiazine compounds, the paucity of research into the properties of these drugs, and the resulting ambiguity surrounding their relative merits, side-effects and specific action suggested the urgent need for a controlled multiple drug study restricted to consideration of phenothiazine compounds. Theoretical understanding of drug action could not advance until a conceptual framework was developed within which the chemical structure of drugs might be related to intervening variables between drug intake and drug action. A controlled quantitative experimental study comparing available phenothiazine compounds would do much toward fixing pharmaceutic reference points, and thus leave fewer unknowns in the search for parameters of drug action.

The purpose of this paper is to present, in part, the results of Comparative Phenothiazine Project MY-2152. In connection with the major purpose of investigating drug action, a number of related problems were considered. These included a study of the influence of physicians' attitudes on their use of drugs (10); the development of a predictive index of treatment effects which would increase the precision of the comparative drug experiment (9); a comparative evaluation of somatic side-effects of the phenothiazine compounds (15); and a study of the pre-admission drug treatment of psychiatric hospital patients (14). Research is being continued on several other specific problems related to Project MY-2152. The present paper is focused on the following primary goals of the study: 1) a determination of the differential effects (inter-drug and drug-placebo) of various phenothiazine derivatives on total morbidity as measured by standard behavioral scales, and 2) a determination of the differential effects of various phenothiazine derivatives on severity of illness as measured by global psychiatric ratings.

PROCEDURE

SUBJECTS

The subjects were newly admitted patients of both sexes between the ages of 18 and 65 who were considered to be candidates for tranquilizing drug therapy by their ward physicians in consultation with the research psychiatric staff. Subjects were referred on the basis of such target symptoms as anxiety, agitation and restlessness, without reference to the usual nosologic considerations, the resulting population being predominantly schizophrenic in character. The actual ages of the subjects ranged from 18 to 61 years, with a mean age of 39 years. The ratio of men to women was 1:2. Patients excluded from the study were alcoholics, court orders, chronic brain syndromes, major organic diseases, and seniles.

CRITERION MEASURES

Criterion instruments included standard rating scales of patient behavior and symptoms, a checklist for recording ward behavior and impressions, and psychiatric rating scales designed to quantify clinical observations. Specific measuring devices were:

1) The *Multidimensional Scale for Rating Psychiatric Patients,* MSRPP (16). This instrument, used to measure total morbidity, consists of 62 rating scales, 22 to be completed by a ward observer and the re-

maining 40 by a clinician on the basis of the one-hour interview with the patient. In addition to the total morbidity score, the MSRPP also yields scores for each of 11 dimensions of psychopathology, for four pattern scores (Normality, Catatonic, Paranoid and Hebephrenic), and for four second-order factors (Thinking Disorder, Withdrawal, Paranoid Belligerence and Agitated Depression).

2) The *Psychotic Reaction Profile* (PRP). This instrument, used to record patient behavior as it appeared to ward personnel, was an experimental form of an inventory developed by Lorr *et al.* (17), and consisted of 141 items of symptoms and behavior to be checked by ward observers when describing a particular patient. The PRP yielded (cluster) scores in four major areas of pathology: Withdrawal, Belligerence, Depression and Thinking Disorder.

3) The *Psychiatric Scale of Target Symptoms.* A set of scales was devised by participating psychiatrists at Spring Grove State Hospital to record clinical observations with greater uniformity and objectivity than were obtained in customary progress notes. These scales were focused on target symptoms. They also included a global evaluation of severity of illness, consisting of the combined ratings (on five-point scales) for each of two variables, Discomfort and Adaptability. *Discomfort* included anxiety, depression, agitation, restlessness and "the subjective element of anger." Ratings of *Adaptability* were based on answers to the following questions which the physician posed to himself: 1) To what extent does the individual function up to his capacity? 2) To what extent does he live up to the expectations of his environment, provided they are realistic? 3) To what extent can he utilize the environment for the satisfaction of his needs and be well received by the environment? The definition of mental health underlying the psychiatrists' global severity of illness rating might be stated as *the ability of the individual to meet environmental demands without disintegrating inner stress.* Each patient was evaluated on the basis of what might be expected if he were to be discharged at the time of the psychiatric interview.

Inter-rater reliabilities among personnel involved in the study were determined for both the MSRPP and the PRP. Prior to official acceptance of patients for study, ward observer inter-rater agreement on the PRP was determined for six pairs of raters from the six wards of the admission service included in the comparative drug project. Each pair of raters observed the behavior of 20 patients for a period of one week (except in one case, where the rapidity of patient movement during the observation period permitted the rating of only 15 patients). The average inter-rater percentage agreement figures found were therefore based on a total sample of 115 patients. Mean intraclass correlations for the PRP scales among 12 evaluators were found to be: Thinking Disorder, .81; Withdrawal, .90; Belligerence, .83; Depression, .59. The range in average percentage agreement for the six pairs of raters on the total 141 checklist items was from 74 to 94, with a mean percentage agreement of 83. Inter-rater agreement for the ward observation section of the MSRPP was also determined for the six pairs of raters on the total sample of 115 patients. The average percentage of complete agreement for MSRPP ward observation items was 69. (Item 51, which could not be scored on all wards, was dropped from the comparison.) Average percentage agreement within one scale point was found to be 91, while that within two points was 96. For 2.8 per cent of the items, one or another of the raters had utilized the "unratable" category, whereas the other had utilized a scale number.

Reliability for the four MSRPP interview raters utilized in Project MY-2152 was determined for a sample of 15 patients, representing varying degrees of illness and selected from the active treatment wards

of the hospital. Ratings were based on single interviewing sessions, during which an alternatingly assigned member of the rating team took the lead in questioning the patient. The average intraclass correlation for the total morbidity score (interview section) of the MSRPP was found to be .75 for the four participating clinical psychologists. For MSRPP interview factors the average intraclass correlation of these raters was as follows: C, .76; E, .72; F, .77; G, .60; J, .80; K, .74.

For the *Psychiatric Scale of Target Symptoms,* inter-rater agreement was examined in practice trials on 20 patients. The two participating psychiatrists jointly interviewed these patients and rated them independently on the target symptoms scale, later discussing their ratings on each patient, with special attention to those on which they had disagreed. In these discussions, mutually acceptable definitions of target symptoms were formulated in the manner described previously in connection with the Discomfort and Adaptability scales. Though formal reliability studies of the *Psychiatric Scale of Target Symptoms* were not made, the effectiveness of this instrument in subsequent differentiation among drug categories indicated an adequate stability of ratings.

DRUGS

The following drugs were studied, using a double-blind technique: promazine (Sparine), chlorpromazine (Thorazine), mepazine (Pacatal), trifluprομazine (Vesprin), prochlorperazine (Compazine) and perphenazine (Trilafon). Phenobarbital (active placebo) and an inert placebo (saline/lactose) completed the eight categories to which patients were assigned.

Medications were given to patients on a double-blind basis. The choice of the particular drug to be used in any case was based on a predetermined random selection of the compounds constituting the eight categories to be studied. The drugs were dispensed in standard unmarked capsules of ℵ0 and ℵ1 size, colored pink to mask all identifying consistencies and colors of the drugs. Capsules were of two strengths, ℵ1 and ℵ2. The ℵ2 capsules contained a dosage double that of ℵ1. The physician knew a patient had six chances in eight of being given a phenothiazine compound, but did not know which drug was being given, or whether the patient was receiving one of the placebos.

A dosage chart listing minimum parenteral and oral dosages for each of the phenothiazines and the two placebos is presented in Table 1. Parenteral dosages are those in standard use, and oral dosages are those suggested by the respective pharmaceutic companies as generally applicable to large patient populations.

METHOD

As each newly admitted patient was assigned to a ward physician, the latter was required to make a decision as to whether the patient was to be placed on a tranquilizing drug. The criterion was the presence of target symptoms (*viz.,* anxiety, agitation, hostility), as delineated in orientation conferences between doctors and research personnel. If the psychiatrist's decision was in the affirmative, he was required to notify the Research Department and to allow 48 hours to elapse so that the Department might obtain necessary evaluations and assign medication.

After preliminary correspondence with the drug companies, the following procedure was decided upon as the optimal method for administering the various medications: After a 48-hour drug-free evaluation period, medication was given parenterally for two days. It was provided in unmarked ampules containing 1 cc., and the minimum dosage was 1 cc. given intramuscularly three times a day. Medication was given parenterally for the first two days of treatment to insure that each patient received the medication and to allow for the

TABLE 1

Comparative Dosage for Phenothiazine Study*

Drugs	Single Dosage Units			Specified Minimum Daily Dosage		Actual Daily Dosage Used in Study		Aver./Min.
	Parenteral 1 cc. Ampule	Oral		Parenteral	Oral	Highest	Average	
		Capsule #1	Capsule #2					
Perphenazine	5	8	16	15	24	96	30.83	1.28
Prochlorperazine	5	10	25	15	30	125	45.38	1.51
Trifluprom azine	25	25	50	75	75	300	110.46	1.47
Mepazine	25	25	50	75	75	450	135.45	1.81
Chlorpromazine	25	100	200	75	300	1200	401.35	1.34
Promazine	50	100	200	150	300	1600	438.92	1.46
Phenobarbital	65	32.5	65.0	195	97.5	360	183.64	1.88
Placebo	Saline	Lactose		3 amps	3 #1 caps.	16 #2 caps.	3 #2 caps.	2.00

* Dosages of all drugs expressed in milligrams.

† Ratio of average to specified minimum daily dosage.

most effective build-up of medication in acutely disturbed patients. On the patient's third day of drug therapy, oral medication was started. The regulation of medication at this point called for a minimum of three capsules a day, which provided in all cases the minimum dosage level in milligrams recommended by the respective drug companies. If after a few days' trial the physician felt dissatisfied with the results of this dosage and wished to increase it, he could change over to the #2 capsule (double the dosage of #1). The physician could prescribe as many #2 capsules as he saw fit, with no predetermined ceiling, until he finally settled upon a dosage which appeared to be producing maximum therapeutic results on the basis of his daily clinical observations of the patient.

It was felt that the above method of administration would allow for thorough exercising of the physician's clinical acumen in setting the particular dosage level which would give the most beneficial results for each individual patient. The ward physician could also drop a patient from the study if he felt he should alter the plan of treatment because of the ineffectiveness of a drug. Whenever drug therapy was discontinued the Research Department was notified and a reason given for the change. In all cases, physicians were urged to keep the patients on drugs as long as possible. Whenever physiologic complications due to the drug developed, this was also brought to the attention of the Research Department, so that an accurate account of such complications might be made. The cut-off point of the study was six weeks from the date medication was started. If the patient was still in the hospital and on the experimental drug at the end of this period, his physician could continue medication if he wished. A note was entered in the patient's chart as to the drug he had been receiving. This information was secured from the pharmacist and recorded in the patient's chart by a member of the nursing staff.

Patients were evaluated initially (pretreatment) in terms of all criterion measures. Subsequently, psychiatric rating scales and the ward observation checklist (PRP) were completed weekly. MSRPP ratings were made at two-week intervals. As indicated previously, the physician could stop treatment at any time in the course of the experimental procedure if he felt there was clinical justification for this move. If a pa-

tient had received medication for at least ten days when his physician decided to remove him from the project, and if one week had elapsed since his previous rating, he was rated again before the drug was discontinued. All clinical and research personnel working with patients were bound to the double-blind stipulation imposed by the research plan.

ANALYSIS OF DATA

Criterion instruments yielded scores for subjects in each of 31 variables. These included an MSRPP total morbidity score, MSRPP factor and pattern scores, PRP cluster scores, psychiatric target symptom ratings and global psychiatric evaluations. To avoid unwieldiness, excessive length, and over-complexity in presentation of results, the present paper will be limited to reporting differences in efficacy of the drugs as measured by the MSRPP total morbidity scores and by psychiatrists' global evaluations of adjustive capacity. Differential effects in specific areas of pathology, as measured by factor scores of the MSRPP and other instruments, will be discussed in a subsequent paper.

The psychiatrists' global severity of illness initially included ratings of both Adaptability and Discomfort. For reasons which will be discussed later, the Discomfort scale was omitted from the criterion instrument in treatment of data reported in this paper. Adaptability as defined previously (*i.e.*, integrative potential or ego strength) was taken as the psychiatrists' estimate of overall emotional health. Thus two criterion measures of global severity of illness, the MSRPP total morbidity score and the psychiatrists' Adaptability rating, were analyzed for differences among drug groups after two weeks of medication and at the end of treatment, whenever that occurred for each patient. Those patients on whom data were incomplete, together with those not receiving medication for at least ten days, were excluded from the study.

The analysis of multiple covariance technique was used to control relationships between initial scores in each of the two measures and post-treatment MSRPP total morbidity. When the Adaptability rating was taken as the criterion measure, analysis of covariance was used to adjust drug group means to approximately equal starting levels in that variable only, since controlling relationships between initial MSRPP total morbidity and final Adaptability score were found to increase error variance and tended to obscure rather than to sharpen post-treatment drug group differences. Relationships between initial and subsequent scores of the individual drug groups in each criterion instrument did not depart significantly from linearity at the two-week evaluation point or from beginning to end of treatment. Regression lines of subsequent initial scores of each drug category were not significantly different from each other in the case of either measure over either of the two time intervals. The Bartlett (2) test for homogeneity of variance was made, and suitable transformations were employed where heterogeneity of variance was revealed. When an analysis indicated significant differences among drug groups, the multiple-range test (6, 12) was used to determine the significance of differences in individual drug comparisons.

RESULTS

Results of the analysis of multiple covariance for the MSRPP criterion measure and the analysis of covariance for the Adaptability rating for the two-week treatment period are presented in Table 2. Comparable results for the entire period of treatment (from beginning to termination) are given in Table 3. Significant differences in efficacy as measured by each criterion among individual drug comparisons over the two-week evaluation period and from beginning to end of treatment are summarized in Table 4.

The rank orders of post-treatment drug

TABLE 2

Two-Week Evaluation (N = 187)

Drug Group	Criterion Measure: MSRPP Total Morbidity						Criterion Measure: Psychiatric Rating of Adaptability			
	Initial Means*		Final Mean	Final Transformed Mean	Final Adjusted Transformed Mean	Rank	Initial Mean	Final Mean	Final Adjusted Mean	Rank
	$\bar{X}_1$	$\bar{X}_2$								
Perphenazine	41.44	3.00	23.89	4.81	4.65	2	3.00	2.39	2.31	4
Prochlorperazine	32.33	2.92	23.46	4.81	4.96	4	2.92	2.13	2.12	3
Trifluprazine	37.00	2.92	24.48	4.79	4.80	3	2.92	2.00	1.99	2
Mepazine	40.05	2.65	32.70	5.56	5.54	6	2.65	2.45	2.68	8
Chlorpromazine	39.35	3.04	23.26	4.70	4.60	1	3.04	2.04	1.93	1
Promazine	34.17	2.88	29.25	5.28	5.38	5	2.88	2.33	2.36	6
Phenobarbital	37.48	3.17	34.30	5.64	5.56	8	3.17	2.57	2.34	5
Placebo	37.69	2.69	31.69	5.50	5.55	7	2.69	2.34	2.53	7
	Multiple Covariance F-ratio = 2.95 df = 7,177 $p < .01$						Covariance F-ratio = 2.48 df = 7,178 $p < .05$			

* $\bar{X}_1$ = Initial Total Morbidity Mean; $\bar{X}_2$ = Initial Adaptability Mean.

TABLE 3

Initial to Final Evaluation (N = 187)

Drug Group	Criterion Measure: MSRPP Total Morbidity						Criterion Measure: Psychiatric Rating of Adaptability			
	Initial Means*		Final Mean	Final Transformed Mean	Final Adjusted Transformed Mean	Rank	Initial Mean	Final Mean	Final Adjusted Mean	Rank
	$\bar{X}_1$	$\bar{X}_2$								
Perphenazine	41.44	3.00	24.89	4.91	4.74	4	3.00	2.22	2.15	4
Prochlorperazine	32.33	2.92	21.21	4.42	4.57	1	2.92	2.04	2.03	3
Trifluprazine	37.00	2.92	22.58	4.62	4.62	3	2.92	1.81	1.80	1
Mepazine	40.05	2.65	30.45	5.63	5.34	6	2.65	2.30	2.49	7
Chlorpromazine	39.35	3.04	23.26	4.71	4.59	2	3.04	2.04	1.95	2
Promazine	34.17	2.88	25.54	4.88	4.99	5	2.88	2.21	2.23	5
Phenobarbital	37.48	3.17	35.17	5.74	5.62	8	3.17	2.70	2.51	8
Placebo	37.69	2.69	31.28	5.34	5.41	7	2.69	2.31	2.47	6
	Multiple Covariance F-ratio = 2.95 df = 7,177 $p < .01$						Covariance F-ratio = 2.30 df = 7,178 $p < .05$			

* $\bar{X}_1$ = Initial Total Morbidity Mean; $\bar{X}_2$ = Initial Adaptability Mean.

group means for each criterion variable are presented in Tables 2 and 3. Rank order correlations between relative efficacy of the drugs according to mean post-treatment MSRPP total morbidity scores and mean post-treatment Adaptability scores were .76 at the two-week evaluation point and .88 at the end of treatment, both significant at the .05 level. There is evidence to indicate that the extent of these correlations is not due to the fact that pre-treatment Adaptability scores were used in adjusting both criterion measures. When MSRPP total morbidity scores were adjusted in terms of initial morbidity scores only, the resulting correlations between criterion means remained unchanged. By contrast (and as an example of the relative independence of the initial drug group means of these measures), the rank order correlation between

TABLE 4

Individual Drug Comparisons: MSRPP Total Morbidity and Psychiatric Adaptability Rating*

Potency Rank	Drug	Criterion Measure: MSRPP Total Morbidity				Criterion Measure: Psychiatric Adaptability Rating			
		Initial to Two Weeks		Initial to Final		Initial to Two Weeks		Initial to Final	
		$\bar{Y}$†	Mean Comparison	$\bar{Y}$	Mean Comparison	$\bar{Y}$	Mean Comparison	$\bar{Y}$	Mean Comparison
1	Perphenazine	4.65	Better than: Mepazine Promazine Phenobarb. Placebo	4.74	Better than: Phenobarb.	2.31	—	2.15	—
2	Prochlorperazine	4.96	—	4.57	Better than: Phenobarb. Placebo	2.12	Better than: Mepazine	2.03	—
3	Triflupromazine	4.80	Better than: Mepazine Phenobarb. Placebo	4.62	Better than: Phenobarb. Placebo	1.99	Better than: Mepazine Placebo	1.80	Better than: Mepazine Phenobarb. Placebo
4	Mepazine	5.54	—	5.34	—	2.68	—	2.49	—
5	Chlorpromazine	4.60	Better than: Mepazine Promazine Phenobarb. Placebo	4.59	Better than: Phenobarb. Placebo	1.93	Better than: Mepazine Placebo	1.95	—
6	Promazine	5.38	—	4.99	—	2.36	—	2.23	—
7	Phenobarbital	5.56	—	5.62	—	2.34	—	2.51	—
8	Placebo	5.55	—	5.41	—	2.53	—	2.47	—

* Multiple range test (12) using .05 level of significance.

† $\bar{Y}$'s equal adjusted means in all cases.

pre-treatment mean scores for MSRPP total morbidity and Adaptability was found to be only .03.

At the two-week evaluation point, the mean total morbidity scores of the perphenazine, trifluprom azine and chlorpromazine groups were significantly lower than those of the mepazine, phenobarbital and placebo groups. In addition to these differences, the mean total morbidities of the perphenazine and chlorpromazine patients were significantly lower than the mean morbidity score of the promazine subjects. The psychiatrists' rating of Adaptability reflected a similar pattern of drug differences after two weeks of treatment. Trifluprom azine and chlorpromazine patients were rated as significantly more adaptable than mepazine and placebo patients. Psychiatrists also rated the prochlorperazine group as more adaptable than the mepazine group.

When the entire period of treatment for each patient was taken as a basis for comparing relative efficacy of the drugs, the MSRPP total morbidity score reflected the superiority of perphenazine, prochlorperazine, trifluprom azine and chlorpromazine to phenobarbital. Of these four drugs, all except perphenazine were also measurably superior to the placebo. Pre-treatment to post-treatment comparisons in the Adaptability scale indicated the superiority of trifluprom azine over mepazine, phenobarbital and the placebo. Differences among other drugs failed to reach the predetermined significance level.

In summary, the drugs studied tended to fall into two groups in the extent to which they reduced MSRPP total morbidity: perphenazine, prochlorperazine, trifluprom azine and chlorpromazine were therapeutically more effective than were mepazine.

promazine, phenobarbital and the placebo. With the exception of perphenazine, the psychiatrists' Adaptability scale placed the drugs in similar groups: trifluprομazine and chlorpromazine were more effective in increasing Adaptability than were mepazine and the placebo; prochlorperazine heightened Adaptability more than did mepazine. Mepazine and promazine were no more effective in reducing total morbidity or increasing Adaptability than were phenobarbital and the placebo in terms of either criterion measure over either time interval.

Individual drugs varied in effectiveness according to the period of administration and the criterion measure. MSRPP total morbidity was reduced more slowly by prochlorperazine than by the other three therapeutically effective phenothiazines. Perphenazine was effective, according to MSRPP ratings, at two weeks; however, the therapeutic value of this drug was less clearly demonstrated at the final evaluation point. In terms of psychiatrists' ratings of Adaptability, perphenazine showed no superiority to any other drug over either time interval. Across the board, so to speak, trifluprομazine and chlorpromazine made impressive showings, demonstrating consistent superiority to phenobarbital and placebo over both evaluation periods.

TABLE 5

Number of Subjects in Each Drug Category at Subsequent Evaluation Points

Drug Group	Referred N	Initial* N	2 Weeks N	4 Weeks N	6 Weeks N	Mean Days Treated N
Perphenazine	36	31	18	11	4	27.11
Prochlorperazine	32	29	24	14	10	29.29
Trifluprομazine	36	31	26	20	12	32.38
Mepazine	34	27	20	11	7	26.70
Chlorpromazine	33	32	23	13	7	27.30
Promazine	32	29	24	15	8	29.92
Phenobarbital	37	29	23	8	2	21.43
Placebo	37	30	29	16	9	26.86
Totals	277	238	187	108	59	27.62

* Number of patients at time of initial evaluation. Subsequent to referral, 13 patients were found to violate population restrictions; 18 were considered poor candidates for tranquilizing therapy by the research staff; and eight were considered as unratable and/or unmanageable.

DISCUSSION

The Sample: Effects of Attrition: The data collection phase of Project MY-2152 began in April, 1958, and continued through November, 1959. From approximately 2,500 patients admitted during this time, 277 patients were selected from admission service as candidates for phenothiazine therapy. Of the 238 patients who met all population restrictions, 187 remained in the project at least ten days and received pre-treatment and post-treatment evaluations on all criterion measures. Fifty-nine patients completed the prescribed six-week treatment course, and 108 remained in the study for approximately a four-week period. The number of patients in each drug category at successive evaluation points is given in Table 5. While only seven per cent of the phenobarbital and 13 per cent of the perphenazine patients completed six weeks of treatment (most of the former being taken off medication because of "no improvement," and the latter because of neurologic complications), 39 per cent of the trifluprομazine patients remained until the predetermined cut-off point. Percentages for the other drug categories ranged from 22 to 34. In terms of average number of days treated, phenobarbital (21.43) and trifluprομazine (32.38) are again at opposite extremes.

An analysis of factors responsible for physicians' failures to refer patients to the study and for subject attrition has been presented in an earlier paper (13). A survey of admission records during the first 18 months of the intake of research subjects revealed that 125 patients who appeared to be suitable for the project had not been

referred. Of these patients, more than half were described as "too disturbed." In connection with interpretation of results, it is important to note here that our sample was curtailed to some extent by a systematic elimination of highly disturbed patients. These patients, who might have been expected to respond well to drugs, tended not to be referred to the study because physicians were unwilling to withhold medication from them for the necessary 48-hour evaluation period.

Selective factors operating in subject attrition led us to discard four-week and six-week treatment comparisons of drug groups. Since physicians could remove from the study patients who showed no improvement, it was felt that the sample was distorted and no longer random after an appreciable number of subjects had been removed from the study on the basis of their individual responses to the drugs. Eighty-one subjects were removed from the study because of lack of improvement, as compared to eight patients who no longer needed drugs and 27 who developed side-effects. The rank order correlation between number of subjects in each drug category removed because of lack of improvement and the potency of the drug (in terms of recommended dosage) was found to be −.96, significant at the .01 level (13). The procedure of selectively removing "failures" from the patient sample thus favored least potent drugs, so that spontaneous remission may be suspected in those patients receiving these drugs who remained in the study. In view of these considerations, comparisons among drug groups at the two-week evaluation point and from beginning of treatment to termination (whenever that occurred in each individual patient) appear to be the most meaningful approach to the problem of valid measurement of drug action when individual reactivity must be taken into account. Phenothiazines can then be compared not only on the basis of a standard time interval with a relatively intact sample of patients, but also in terms of a flexible period of administration tailored to individual response.

Somatic Reactions: A total of 25 patients receiving active drugs were removed from the study because of neurologic or other somatic complications. Of the 14 subjects who showed neurologic reactions, six developed symptoms during the first two days of medication (in the period of parenteral administration), nine during the first five days, and 11 by the tenth day. Neurologic symptoms were divided among the drugs as follows: perphenazine, eight patients; trifluprom azine, four patients; prochlorperazine and promazine, one patient each. Vasomotor episodes were the most frequently observed non-neurologic symptoms. Of the 11 patients showing somatic disturbances other than central nervous system reactions, six patients (two chlorpromazine, two promazine, one prochlorperazine, one phenobarbital) developed vasomotor episodes, and three patients (two chlorpromazine, one prochlorperazine) had skin rashes. One patient (chlorpromazine) developed leukopenia, and another (mepazine), a visual disturbance. The comparatively high incidence of neurologic complications observed during the first two days of parenteral medication may perhaps be related to Freyhan's (8) findings that the potent phenothiazines act very rapidly, and that all phenothiazines act more potently when given intramuscularly. The number and severity of undesirable drug effects followed roughly the order of potency of the phenothiazines. Perphenazine was by far the worst offender, with eight patients (out of a total for all drugs of 14 patients) removed from the study because of neurologic complications. Trifluprom azine was next, with four patients, and prochlorperazine and promazine were third, with one each. In the order of drug potency, non-neurologic somatic effects tended to begin where CNS symptoms stopped; they did not appear in two of the three most potent phenothiazines

(perphenazine and trifluprομazine). Chlorpromazine patients accounted for five nonneurologic somatic reactions; prochlorperazine and promazine, two each; mepazine and phenobarbital, one each.

Results Compared with Those of Similar Studies: If consideration of comparative multiple psychiatric drug studies is limited to those involving more than four drugs of the same chemical family, we find none published before 1959. Early in that year Freyhan (8) reported a comparative evaluation of ten phenothiazine compounds. In December, 1959, Smith *et al.* (23) published results of a comparison of five phenothiazines. In August, 1960, Casey *et al.* (5) reported a large-scale experiment, carried out co-operatively in a number of Veterans Administration hospitals, in which five phenothiazine derivatives and phenobarbital were compared.

Consideration of these studies has been reserved for this section of the paper for two reasons: 1) Chronologically, they were not published until our study was near completion, hence their results did not enter into the planning of our own experiment. 2) The relatively small number of such studies permits interesting comparisons to be made among methodologies, patient samples, hospital conditions and other variables which might affect results.

The setting of Freyhan's work was the admissions building of a state hospital, and the subjects were acutely disturbed patients selected on the basis of target symptoms which had excitement (as opposed to apathy) in common. The ratio of men to women was 1:2, and patients in the various drug categories ranged in number from 22 to 69. The author's goal was to determine properties of the phenothiazines when treatment was tailored to each patient's "biochemical individuality," and to relate as far as possible the clinical response of patients in treatment to the chemical structures of drugs compared. Certain controls, such as double-blind administration of medication and random selection of patients, were not included in Freyhan's research design in the interest of naturalistic and intensive clinical observation of many facets of individual response to particular drug treatment. Though doctors and nurses were responsible for screening subjects and recording independent daily observations of their behavior, the author personally saw all subjects initially, made final decisions as to their disposition, and served as a consultant regarding their medication. Among phenothiazine compounds of the chlorpromazine model, Freyhan found chlorpromazine and trifluprομazine to be therapeutically successful, with little difference observable in their action. Both were markedly more effective than was promazine, which showed a substantial number of therapeutic failures. In the prochlorperazine model group, perphenazine, trifluprομazine and prochlorperazine showed "marked similarity in action pattern, reflected in the degree of uniformity of the therapeutic results." Mepazine was found to be ineffective therapeutically, and to produce numerous and relatively severe side-effects. Reserpine was less effective than were five out of the six phenothiazine compounds with which it was compared, the exception being promazine. Freyhan reported detailed observations of extrapyramidal reactions and other somatic symptoms associated with each drug, and related these effects to the potency of the compound rather than to dosage.

In August, 1960, the Psychiatry and Neurology Service, Department of Medicine and Surgery of the Veterans Administration, reported results of a large-scale comparative study of five phenothiazine compounds and phenobarbital, carried out co-operatively in 35 Veterans Administration hospitals (5). Differences in milieu were partially controlled by the stipulation that each drug be represented an equal number of times in each hospital. Each participating hospital was requested to furnish 18

subjects, to be recruited within a period of three months. Responsibility for carrying out research proceedings rested with one staff member of each hospital participating in the study. The final sample of patients studied consisted of 640 newly admitted schizophrenic men between the ages of 18 and 54 years, of whom 30 per cent were married. The subjects had of course served in the Armed Forces, and either had first developed mental illness in the service or had had an existing condition exacerbated by the service. The MSRPP and a clinical estimate of psychiatric status were used to measure changes which occurred in treatment. Double-blind procedures were followed in administration of drugs, and patients were assigned randomly to treatment groups. Results of the Veterans Administration study indicated that all five phenothiazine derivatives were therapeutically more effective than was phenobarbital. Mepazine was less successful in reducing total morbidity than were the other four drugs at the dosage employed. No significant differences in efficacy were noted between chlorpromazine, trifluprom azine, prochlorperazine and perphenazine. Criterion measures showing change toward improvement after treatment with phenothiazine derivatives included resistiveness, belligerence, thinking disturbance, and degree of illness. Other criteria affected favorably, especially by the four more potent phenothiazines, were motor disturbances, paranoid projection, perceptual distortion, and withdrawal.

In contrast to results of the above studies are those of Smith *et al.* (23), who in November, 1959, reported the results of a comparative study of five phenothiazines. Subjects were chronic patients (63 men, 72 women) who had been refractory to previous treatment. Willingness to take oral medication was one criterion for patient selection. The authors concluded that improvement was so slight on many of the drugs that further study, using double-blind controls, seemed unnecessary.

Results of the present experiment are consistent with those of Freyhan and the Veterans Administration study, as well as those of clinical studies such as Ayd's (1); fluphenazine, prochlorperazine, chlorpromazine and trifluprom azine were consistently found to be therapeutically more successful than were phenobarbital, a placebo and mepazine. Freyhan and Ayd observed that promazine was less effective than were chlorpromazine, trifluprom azine, prochlorperazine and perphenazine. Since promazine had previously been shown to be less effective than chlorpromazine (4), it was not incorporated in the Veterans Administration multiple phenothiazine research. Our results indicated that perphenazine and chlorpromazine were more effective after two weeks of treatment in lowering total morbidity than was promazine, and that promazine was not rated better than phenobarbital or placebo in any comparison.

Such agreement as to the comparative action of the phenothiazine compounds is in itself a finding of some significance. Differences in therapeutic success between effective and ineffective phenothiazines, and between the former and phenobarbital and placebo, seem to be sufficiently gross to be apparent regardless of differences in experimental approaches, settings, and types of patients studied. Results reported by Smith *et al.* (23), whose subjects were chronic "untreatable" patients, furnish a sole exception to the general consensus of other investigators. Our results are of interest in that comparative efficacy of the phenothiazines at the two-week evaluation point was substantially the same as that found in the Veterans Administration study after a one-month treatment interval. With the exception of prochlorperazine, all of the effective drugs acted more rapidly than might have been expected from reports in the literature. If patients were going to improve at

ill, they were measurably better after two weeks of treatment. At present we are examining patients after five days of phenothiazine therapy; trends in the data generally point toward significantly lowered total morbidity at this point.

There was also some indication from our data that after an initial rapid improvement, patients taking some of the more potent drugs began to appear worse to MSRPP raters. The mean total morbidity of the perphenazine group was higher at the final evaluation than at two weeks. Psychiatrists also saw this drug as no more effective than were phenobarbital and the placebo over either time interval. Since the psychiatrists' ratings were based on the patients' potential for adjustment in the community, they may have been influenced by disabling somatic effects to such an extent that improvement in underlying pathology was not noted. It is not clear why short-term gains in the perphenazine groups in terms of lowered MSRPP total morbidity tended to be lost in the final evaluation, unless, as Freyhan suggested, efficacy of the phenothiazine drugs is correlated with the appearance of undesirable somatic complications. If such is the case, perhaps a disproportionately large number of perphenazine patients who might have shown improvement with a lower dosage, or with extrapyramidal symptoms controlled by medication, were removed from the study because of alarming neurologic reactions. It is also possible that side-effects tended to mask whatever improvement occurred. A drug which slows down a patient's reactions, makes him appear groggy, or produces rigid posture and gestures, may mimic functional illness to such an extent that symptoms of the two conditions are indistinguishable. It is in this context that we can perhaps best interpret the finding that patients taking a drug which produced "zombi-like" muscular rigidity appeared to get progressively worse after some initial improvement in more blatant psychotic symptoms.

METHODOLOGIC SIDELIGHTS

The Statistical Model: The focus of the present paper has been on differential drug effects indicated by significant differences in drug group post-treatment means in measures of overall pathology. It was necessary to omit the psychiatrists' rating of subjective Discomfort from these major criteria, because relationships of subsequent to initial scores in this variable were significantly different among the eight drug categories. The F-ratio of means is an exact test of significance only when treatment groups compared show homogeneity of variances and regression coefficients. The finding of heterogeneity of variances and/or regression coefficients in the data may in itself furnish valuable information relative to differential drug effects. We are at present analyzing data further in order to interpret such findings meaningfully in terms of the clinical problem of prescribing medication best suited to the symptoms of an individual patient. Differential efficacy in the treatment of patients initially manifesting varying degrees of the criterion measure (*e.g.*, Discomfort) can be hypothesized from differences in regression coefficients, though the data may not permit valid comparisons of drug group means (since these groups include patients of all levels of initial Discomfort). Indications that a patient's initial level of Discomfort may influence whether he will be made more comfortable or less comfortable by a given phenothiazine can perhaps be related to Freyhan's observation that the four more potent compounds were less successful with psychoneurotic patients than with psychotic, as these drugs had in common a tendency to produce akathisia. Such symptoms in mildly ill patients were regarded as an indication of therapeutic failure, whereas they were relatively unimportant in the

light of overall improvement observed in severely psychotic subjects.

To use the analysis of covariance method to differentiate between drug effects in terms of post-treatment differences among drug group means, it is necessary that certain underlying assumptions be fulfilled. Computer programs frequently do not provide a means for determining whether data meet these assumptions. For this reason, after our data were processed by computer, significant F-ratios were then computed by hand calculator, testing at various intermediate steps for appropriateness of the technique. We found that data from the MSRPP total morbidity criterion variable had to be further processed before the analysis of covariance technique could be used, and data from still other measures could not be treated in any way which would satisfy requirements imposed by the method. We therefore feel that a word is in order here regarding the analysis of covariance as a statistical model for comparative drug studies.

The analysis of covariance F-ratio of means, though the best method presently available for differentiating among treatments when the individual's initial status in the criterion measure influences outcome, has fairly serious drawbacks if relied on to do more than screen treatment comparisons for closer examination. Restrictions imposed on the data may serve to mask relationships which are as relevant to the problem of comparative treatment evaluation as are final results in terms of differences among drug group means. Further, investigators may be seduced by a computer's neat columns of print, with categoric answers in terms of F-ratios of means, to the extent that they omit the laborious examination of data to determine whether assumptions basic to the technique have been met.

One condition which must obtain if the F-ratio of means is to be a precise criterion of differences is that the data show homogeneity of variance, *i.e.*, that the individuals in a given drug group not be more like each other in the characteristic measured than are those in another drug group. If this assumption is violated in the post-treatment data, then the data must be "adjusted" in order to make drug groups similar in amount of scatter among individuals. Now, whether because of artifacts of measurement, or because of the nature of reality, post-treatment groups in studies involving attitudes and social behavior of human beings rather frequently show heterogeneity of variance, with individual variation within a group related to the average score of the group. In our MSRPP data, the "sicker" the post-treatment group, the more they differed among themselves. Since this was not the case in the Adaptability scale analyses, it is possible that the MSRPP total morbidity scale may be compressed near the zero point. In other words, since there is no "positively healthy" pole, changes in patients who are not very ill may not be measured adequately. The calibration of the scale may be too gross to reflect changes near the "normal" range, so that these patients cluster at the zero point, with nowhere to go in the positive direction. On the other hand, a case might be made for regarding the relationship between degree of illness and similarity to others as reality. One might say the sicker a group is, the more variety there will be in the individuals it comprises.

Another assumption underlying use of the analysis of covariance F-ratio of means is that the data show homogeneity of regression—that is to say, the relationships between final score in the criterion variable and initial score must not be significantly different for the treatment categories involved. Though most of our data did not show heterogeneity of regression at a statistically significant level after adjustment was made for heterogeneity of variance, certain information about drug action was revealed by a comparison of regression lines of individual drugs. For instance, in the MSRPP total morbidity cri-

terion analysis of multiple covariance, there was very little relationship between initial and two-week scores in the perphenazine group of patients. Whatever the pretreatment severity of illness of each individual, patients who remained on the drug for two weeks were at very nearly the same post-treatment level of morbidity. There was some evidence in the data of a tendency for the groups taking the more potent drugs to show less relationship between final and initial morbidity than did groups taking the less effective phenothiazines, phenobarbital and placebo. This relationship, not statistically significant in our data, might be found in greater strength with larger samples of patients. Though reasoning by analogy is treacherous, the action of phenothiazines might be compared to that of antibiotics. If a patient has fever from an infection, the height of his temperature before receiving an antibiotic bears no relationship to his post-treatment temperature. If, on the other hand, he received symptomatic treatment (aspirin or cold compresses), the initial severity of the fever might be related to the final temperature reading.

Though admittedly speculative, the above is offered to counteract the spurious feeling of finality which comes of deriving magical numerical answers to still complex and obscure problems. In the preceding paragraph it was our intention to point out that one must, in a sense, artificialize data from comparative treatment studies to fit them to the most nearly appropriate statistical method. The tail is uncomfortably close to wagging the dog. We do not have a superior method of data analysis to offer at this time. We do feel, however, that results of the analysis of covariance procedure cannot be adequately interpreted without taking the above considerations into account.

The Clinician: The clinical flexibility built into the design of MY-2152 provided interesting secondary criteria of drug action. As noted above, patients were taken off drugs in approximately the reverse order of the potency of the drugs as indicated by dosages recommended by the respective drug companies. Table 1 contains the ratio between average daily dosage of each of the drugs and the minimum specified by the research design. The physicians, allowed to increase dosage as they wished, followed roughly the reverse order of drug efficacy in the extent to which they utilized each of the drugs. In this connection, it can be observed that the average daily dosages of placebo, phenobarbital and mepazine were 2.00, 1.88 and 1.81 times the specified minimum, respectively, as compared to 1.28, 1.51, 1.47 and 1.34 for perphenazine, prochlorperazine, triflupromazine and chlorpromazine. Apparently relatively larger dosages (in terms of drug company recommendations on which research minimum dosage was based) of ineffective drugs were given, so that their lack of success cannot be attributed to dosage.

The comparability of the MSRPP total morbidity score and the psychiatrists' rating of Adaptability in distinguishing between effective and ineffective drugs is of great interest in view of the functional independence of these measures (pre-treatment Pearsonian $r = .40$). The psychiatrists' evaluation was of a complex "multidimensional" quality which might be thought of as ego strength. It was related to community mores as well as to patient pathology. The MSRPP total morbidity score measured degree of illness manifested in clinical interview behavior. The high degree of correspondence between the relative efficacy of the drugs as measured by MSRPP total morbidity and by Adaptability is particularly interesting in that the drugs had changed relative positions at each of the evaluation points according to both criterion instruments. The rank orders of the post-treatment drug group means are presented in Tables 2 and 3 to permit comparison of the relative therapeutic value of

the drugs as determined by two virtually independent criteria. It is understood, of course, that the *order of efficacy* of the phenothiazines, rather than the absolute merit of any one drug, is the focus of interest in considering rank order relationships.

Since the psychiatrists' Adaptability scale proved to be useful in quantifying a dimension not represented in the hospital rating scale, the possibility of combining the two instruments for an even more sensitive criterion measure is suggested. Sociologic variables are known to influence length of hospitalization, stability of remission, and numerous other events in the course of illness that are used from time to time as criteria of treatment effects. The potential of the patient for coping with his social environment tends to be dealt with obliquely and abstractly, in a frame of reference which considers pathology to be an individual matter. A direct evaluation by trained behavioral scientists of the resultant of patient adjustive capacity in meeting environmental stresses may prove to be a valuable addition to prognostic batteries.

SUMMARY

This is the second of a series of three papers reporting on the experiences and results of a comparative phenothiazine study on acute admissions recently completed at Spring Grove State Hospital. It has as its major emphasis global measures of severity of illness.

Two hundred thirty-eight newly admitted, acutely disturbed patients were selected for phenothiazine therapy on the basis of target symptoms. They were then randomly assigned to a six-week treatment course with one of the following: perphenazine, prochlorperazine, trifluprom azine, mepazine, chlorpromazine, promazine, phenobarbital and inert placebo.

The six phenothiazines and two placebos were administered on a double-blind basis. After a 48-hour drug-free evaluation period, medication was given parenterally for two days. On the patients' third day of drug therapy, oral medication was started. Minimum daily dosages of parenteral medication were as follows: perphenazine, 15 mg.; prochlorperazine, 15 mg.; trifluprom azine, 75 mg.; mepazine, 75 mg.; chlorpromazine, 75 mg.; promazine, 150 mg.; phenobarbital, 195 mg.; placebo, three ampules. As with the parenteral medication, the regulation of oral medication called for the minimum dosage of each drug recommended by the respective drug companies. Physicians were allowed to increase dosage as much as they saw fit until they felt they had settled on an amount of the drug which produced optimum therapeutic results. Average daily doses administered and minimum dosages specified were as follows (specified minimum dosage given in parentheses following average daily dosage): perphenazine, 30.83 mg. (24 mg.); prochlorperazine, 45.38 mg. (30 mg.); trifluprom azine, 110.46 mg. (75 mg.); mepazine, 135.45 mg. (75 mg.); chlorpromazine, 401.35 mg. (300 mg.); promazine, 438.92 mg. (300 mg.); phenobarbital, 183.64 mg. (97.5 mg.); placebo, three ℞2 capsules (three ℞1 capsules).

Patients were evaluated initially (pretreatment) in terms of standard rating scales, a ward behavior checklist, and psychiatrists' clinical evaluations. Drug comparisons reported here were based on two measures of global severity of illness: the MSRPP total morbidity score, and a psychiatric judgment of Adaptability. Patients were rated bi-weekly with the former instrument and weekly with the latter. Physicians could remove from the experiment those patients whose response to treatment was unsatisfactory. If a patient remained in the study for at least ten days he was evaluated in terms of criterion measures when treatment was terminated.

For each criterion, the mean of each of the eight drug categories, adjusted to control for initial degree of illness, was com-

pared with the mean of every other drug group after two weeks of treatment and at the end of treatment, whenever this occurred for each individual patient.

Drugs tended to fall into two groups in the extent to which they reduced MSRPP total morbidity: perphenazine, prochlorperazine, triflupromazine and chlorpromazine were therapeutically more effective than were mepazine, phenobarbital and the placebo. With the exception of perphenazine, the psychiatrist's Adaptability scale placed the drugs in similar groups: triflupromazine and chlorpromazine were more effective in increasing Adaptability than were mepazine and the placebo; prochlorperazine heightened Adaptability more than did mepazine. Mepazine and promazine were no more effective in reducing total morbidity or increasing Adaptability than were phenobarbital and the placebo in terms of either criterion measure over either time interval.

Individual drugs varied in effectiveness according to the period of administration and the criterion measure. MSRPP total morbidity was reduced more slowly by prochlorperazine than by the other three therapeutically effective phenothiazines. According to MSRPP ratings, perphenazine was effective at two weeks, but less clearly successful if the entire treatment period was considered. This compound showed no superiority over other drugs or the placebo in terms of psychiatrists' ratings of Adaptability. Triflupromazine was rated as better than mepazine, phenobarbital and placebo in most comparisons. Chlorpromazine came next in unanimity of agreement, showing consistent superiority to mepazine and placebo after two weeks' treatment, and greater efficacy in reducing total morbidity at the final evaluation than placebo and phenobarbital.

Neurologic and other somatic symptoms were recorded during the course of treatment. The number and severity of undesirable drug effects followed roughly the order of potency of the phenothiazines. Eight perphenazine patients (out of a total of 14 patients for all drugs) were removed from the study because of neurologic complications. Triflupromazine was next, with four patients, and prochlorperazine and promazine were third, with one case each. Chlorpromazine patients accounted for five non-neurologic somatic reactions; prochlorperazine and promazine, two each; mepazine and phenobarbital, one each.

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HISTAMINE WHEAL FORMATION AND MENTAL ILLNESS

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The role of histamine in normal and abnormal physiology has received a great deal of study. It is an extremely potent agent, widely distributed in the tissues, from which it may be released by any noxious stimulus, however mild (5), or by specific histamine liberators (25). It is normally assumed to be responsible for the triple response of Lewis. Riley (34) has demonstrated, simply but beautifully, that histamine is located in the mast cells and so is concentrated in areas rich in these cells (connective tissues, serous membranes and skin). Of significant interest in psychiatry is the fact that serotonin has also been demonstrated in mast cells, from which it too is released by histamine liberators (3).

Study of the role of histamine in the physiology of mental illness has centered mainly on two areas: first, its value in determining the relationship between allergy and psychosis; second, its possible importance in treating psychoses. A fundamental relationship has been suggested between the psychoses and, for example, asthma and hay fever, but there is no general agreement (1, 4, 6, 8, 11, 13, 18, 19, 31, 33, 38, 42, 43). Likewise, there is no general agreement on the incidence of clinical allergy in psychotic patients (2, 9, 20, 24, 29, 32, 36, 47, 50). We feel, as do most investigators, that allergic manifestations are much reduced in schizophrenic patients. The discovery of chlorpromazine during a search for an antihistamine drug has, in itself, highlighted the above interests.

The use of intravenous histamine as a therapeutic agent in the treatment of schizophrenia again led to inconclusive results (10, 12, 14–16, 27, 28, 35, 37, 39, 40, 41). Investigators did find, however, that in some instances schizophrenics had an abnormal tolerance to histamine (22). More recently, attempts have been made, using routine skin testing techniques, to investigate the allergic relationship to psychoses and also the histamine sensitivity of psychotic patients. Ermala and Autio (7), using a dilute histamine solution, showed that following intradermal injection the area of reflex erythema was smaller in schizophrenic patients than in a control group. The area of the wheal was not measured in this study. Freedman, Redlich and Igersheimer (9), in an attempt to determine whether schizophrenics are hyporeactive as well as hypoergic, and thus perhaps less likely to manifest allergic symptoms, tested a group of patients with tuberculin and various dilutions of histamine and other allergens. They used a standardized, intradermal administration of the substances, and demonstrated that the patient group showed no diminished allergic reaction as compared with controls, but had consistently smaller wheal responses to histamine. Using a different technique, Weckowicz and Hall (48) devised a weighted score which combined size of wheal and time of appearance of wheal. Initially they claimed the possibility of a skin histamine test for use in the differentiation of schizophrenics and non-schizophrenics, but a later paper was less optimistic (49). Jodrey and Smith (17), in a study of releasable histamine levels and histamine controls, claimed that the releasable skin histamine did not distinguish between clinically normal and psychotic patients. They also found that control subjects and mental patients gave similar responses to intradermally injected histamine. They made no effort to reconcile their findings with those of Weckowicz and Hall, claiming only that their technique of giving hista-

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mine and evaluating the results was quite different. The present investigation set out to examine the vexing questions:

1) Is there a difference in sensitivity between psychotic patients and controls?

2) If there is a difference, has it anything to do with chronicity of the disease?

3) What, if any, is the effect of psychopharmaceutics on the aforementioned results?

PROCEDURE

Two methods for testing the sensitivity of skin to any substance are used routinely by allergists and dermatologists:

1) The skin scratch technique, similar to that applied by Weckowicz and Hall (48, 49).

2) The intradermal injection technique, used by Ermala and Autio (7) and Jodrey and Smith (17). Halpern in Paris mentioned and expressed a preference for a third method for measuring the skin reaction to histamine. This technique followed from the investigations of early workers on capillary inflammation when it was shown that injured capillaries were more permeable to plasma colloids and other fluids. The localization of vital dyes in areas of inflammation was demonstrated as early as 1912 (23), and in 1929 Menkin (30) showed that T-1824, when injected into the circulating blood, rapidly entered the site of inflammation and was fixed there so that the tissues were deeply stained.

In the work about to be reported the third method was used to test skin sensitivity to histamine, as follows:

> Three ml. of one per cent T-1824[2] solution were injected intravenously into one arm of the subject, immediately followed by 0.2 ml. of 1:1000 histamine phosphate solution intradermally into the volar surface of the other arm. The site of the histamine injection was observed and the time noted at the first sign of discoloration. One hour later the subject was seen again; the stained wheal was outlined, traced and transferred to paper, and the area was measured by a standard planimeter. The above technique was adopted after much trial and error. At one hour the wheal is already maximal. In an effort to standardize procedures, all injections and all outlinings and tracings of the wheals were done by the same person (GMS).

[2] The T-1824 (Evans blue) was kindly supplied by Warner-Chilcott.

RESULTS

Table 1 shows the area of wheals in square inches as observed in males, none of whom was receiving medication. Four such groups were studied:

1) Newly hospitalized, first-admission schizophrenic patients.
2) Newly hospitalized patients with a diagnosis other than schizophrenia.
3) Chronic schizophrenic patients, representing those who had been hospitalized for a minimal period of five years.
4) A normal control group, consisting of members of the research staff and of volunteers from the U. S. Army.

Table 1 shows the mean area for the group of chronic schizophrenics to be considerably less than that for any of the groups and the standard deviation to be less than that for any of the other groups.

Table 2 presents a detailed analysis of the results. It is seen that there is a highly significant difference between the chronic schizophrenic groups and all other groups ($p = .02$), but no significant difference between the other three groups. No attempt was made to break down the chronic group according to the duration of illness. (Both Weckowicz and Hall, and Jodrey and Smith, found that there was no significant difference in this respect.) The question of whether age rather than chronicity of illness might be a factor was tested as follows: Acute schizophrenics between 30 and 40

years of age were compared with chronic schizophrenics of the same age group. The sample was small (N = 18), but the difference was significant ($p < .05$). If the same groups were compared with the age range of 30 to 45 years (N = 34), the significance was found to be even greater ($p < .01$). Thus the difference between the groups seems to be related to chronicity of illness rather than to age.

The second part of this experiment was concerned with the effects of certain drugs on the histamine sensitivity of hospitalized chronic schizophrenic patients. This group was selected because it was the only deviant one. The same test was carried out: the patient was placed on appropriate medication at a suitable dosage, and six weeks later the test was repeated. The following drugs were used for the six-week period:

Chlorpromazine (Thorazine), 400 mg. daily. Prochlorperazine (Compazine), 75 mg. daily. Reserpine (Serpasil), 4 mg. daily. Imipramine hydrochloride (Tofranil), 100 mg. daily. Meprobamate (Miltown), 2,400 mg. daily.

Tables 3 and 4 show the results of this part of the experiment. It can be seen that there is a significant difference in the results obtained in the group receiving imipramine and prochlorperazine, but no difference in the group receiving chlorpromazine, meprobamate and reserpine.[3] The results with reserpine were a surprise in view of the findings of Jodrey and Smith, and for this reason the test was repeated. Again no significant difference between the pre-treatment and post-treatment response was found after six weeks of reserpine at a dosage of 4 mg. per day. At the moment the writers can offer no explanation. It would be of interest from this point of view to know the effect of an M.A.O. inhibitor on histamine wheal formation.

TABLE 1

Response To T-1824: Histamine Test By Diagnosis

Diagnosis	Number of Cases	Mean Area of Histamine Wheal in Sq. In.	S.D.	S.E.
Acute Schizophrenics	53	1.42	0.32	0.04
Acute Non-schizophrenics	26	1.43	0.68	0.13
Chronic Schizophrenics	58	1.09	0.24	0.03
Controls	33	1.63	0.98	0.17

TABLE 2

Inter-Group Differences With T-1824: Histamine Test

Groups	Diff. of Mean	*t*
Acute Schizophrenics vs. Chronic Schizophrenics	0.33	6.60*†
Acute Non-schizophrenics vs. Chronic Schizophrenics	0.34	2.43‡
Normals vs. Chronic Schizophrenics	0.54	3.20†
Acute Schizophrenics vs. Acute Non-schizophrenics	0.01	0.07
Acute Schizophrenics vs. Normals	0.21	1.19
Normals vs. Acute Non-schizophrenics	0.20	0.93

* The higher value obtained here is assumed to be due to the larger sample.
† $p < .01$
‡ $p < .02$

DISCUSSION

A significant difference in histamine wheal formation has been found between chronic schizophrenics and all other groups tested. This is in striking contrast to recent reports by other workers, who found either

[3] Recent publications (44, 45) have shown that imipramine is of value in the treatment of hay fever and allergic rhinitis. One of the authors (GMS) successfully treated 12 non-psychiatric patients suffering from allergic rhinitis with this medication after the above tests had been carried out.

TABLE 3

Responses Pre- and Post-Drugs on T-1824: Histamine Test

Drug	N	Mean Area of Histamine Wheal in Sq. In.		SD_1	SD_2	SE_1	SE_2
		Pre-	Post-				
Chlorpromazine	24	1.13	1.10	0.28	0.35	0.06	0.07
Imipramine HCl	28	1.18	0.95	0.41	0.25	0.08	0.05
Meprobamate	23	1.15	1.15	0.35	0.33	0.07	0.07
Prochlorperazine	27	1.16	0.99	0.28	0.23	0.05	0.04
Reserpine	26	1.15	1.05	0.43	0.14	0.08	0.03

TABLE 4

Significance of Pre- and Post-Drug Responses on T-1824: Histamine Test

Group Pre- and Post-	Diff. of Mean	t
Chlorpromazine	0.03	0.37
Imipramine HCl	0.23	2.52*
Meprobamate	0.0	0.00
Prochlorperazine	0.17	2.48*
Reserpine	0.10	1.13

* $p < 0.02$.

that there was no difference between normals and schizophrenics (17), or that there was a difference between schizophrenics and all other groups (48, 49). The latter authors also found a significant difference between acute and chronic schizophrenics. This agrees with our findings.

The most obvious explanation for this difference would seem to be chronicity of the disease process. Weckowicz and Hall were able to show a range in their results associated with acute, semi-chronic and chronic schizophrenics. (That there is a difference between these three groups but no difference between subgroups within the chronic group suggests that an end point is reached somewhere between the second and fifth years of illness.) Age was not a significant factor in their results; this also agrees with our findings. This leaves little doubt that the response was in some way related to the chronicity of the disease. The work of Lovett Doust, Hudson and Salna (21) and Stern *et al.* (46) suggests that schizophrenics have a raised blood histamine level, and thus a diminished skin response might well be a desensitization process. Also pertinent is the discussion by Marazzi *et al.* (26) of a potential histaminogenic mechanism for psychosis.

The contradictory findings could be explained by the techniques employed. Fortunately, this can be tested. Also, obviously, the time chosen for measurement must be compatible with demonstration of the greatest possible differences.

The second part of this study reveals that skin response to histamine is altered by certain of the psychopharmaceutics, namely, imipramine and prochlorperazine. This is again contrary to the findings of Jodrey and Smith, who noted a trend with all drugs used but found that only reserpine produced a statistically significant difference. That the resulting alteration of skin histamine sensitivity is a direct antihistaminic effect of the drugs is strongly suggested by the authors' results and also those of other workers (44, 45), who have found imipramine useful in the treatment of various allergic diseases. In addition, the rapidity of onset of the anti-allergic manifestations of the drugs suggests that the psychic manifestations are not related to the antihistaminic activity. The writers would not claim, however, that the dosages mentioned previously were equipotent.

SUMMARY

Intradermal histamine produced a significantly smaller wheal in chronic schizophrenics than in acute schizophrenics, acute non-schizophrenics or normal controls.

There was no significant difference between the wheals produced in the other three groups.

These findings are discussed in relation to recent contradictory findings, and a possible method for reconciling these findings is suggested.

A possible explanation for this decreased histamine sensitivity is briefly mentioned.

Imipramine and prochlorperazine were found to produce a significant reduction in histamine wheal formation in chronic schizophrenics. (Imipramine was also found to be effective in treating allergies in normal subjects.)

Chlorpromazine, reserpine and meprobamate were not found to have a significant effect on histamine wheal formation.

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PSYCHIATRY AND PHILOSOPHY OF SCIENCE.

II: SOME APPROACHES TO CONCEPTUAL PROBLEMS[1]

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INTRODUCTION

In the first section of this paper (15) we proposed that science be viewed as a spiral where controlled observation alternates with creative conceptualization in a process which will gradually provide greater knowledge of natural events. It was the thesis of the earlier part that the phase of conceptualization, as well as the phase of observation, requires "controls." This is true because scientists are prone to err in the use of language as well as in observing objects and events. The errors of reification and rigidity in conceptualization result in a body of theory which fails to reflect and incorporate new empirical findings. When concepts are seen as ultimate realities and postulates as final laws, newer observations have diminished impact, and alternate theories are often neglected because they are experienced as threats to the security of certain knowledge. On the other hand, the conceptual errors of looseness, vagueness and ambiguity of terms result in a loss of objective empirical meaning for these terms, and a consequent inability to construct empirically testable theory. Because the errors of vagueness and ambiguity are reflected in the failure of scientists to agree in the use of terms, these errors were collectively called the error of unreliability. In the earlier part of the paper it was argued that the error of unreliability, together with errors of reification and inflexibility, constitutes the source of many of the conceptual problems of psychiatry.

In this, the concluding part of the paper, an effort is made to describe some recent trends in psychiatry and to show how they represent efforts to solve the conceptual problems created by reification and rigidity and by unreliability. Existential psychiatry is seen as such an attempt. While unquestionably of value in pointing up errors of inflexibility and excessive reliance upon dogmatically held abstractions, the existentialist position is, in our view, itself guilty of excesses insofar as it abandons scientific method in the study of human behavior.

A second and less extreme reaction to the problems created by inadequate conceptualization has been the loss of confidence in the value of certain specific concepts, particularly of diagnostic terms. Benjamin (3) has pointed out that the practice of disparaging psychiatric diagnosis has been popular for some years. Our discussion will focus on those criticisms which warn of the dangers of the "disease model" in the conceptualization of disordered behavior, and which urge the discard of particular diagnostic terms because they are often reified and employed inflexibly, or because their meanings are vague and ambiguous. The position that such conceptual abuses are obstacles to scientific progress clearly has much in common with the views we expressed in Part I and summarized in the opening paragraph of this part. It seems entirely possible that many current diagnostic terms should be discarded. It is also possible, however, that some of these terms may contain threads of usefulness which will never be exploited unless the two major

[1] This work has been supported in part by a grant from the Duke University Council on Research to the project, "Interview-based Multilevel Personality Diagnosis," of which the senior author is principal investigator and John C. Altrocchi, Ph.D., co-investigator.

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errors of conceptualization are avoided. We therefore suggest conceptual strengthening of terms like "schizophrenia," rather than their immediate discard.

A third trend in the efforts to advance psychiatric knowledge and to surmount conceptual difficulties is directed toward strengthening methodology by the application of scientific controls. If the special requirements of behavioral science are recognized, and the mere imitation of the techniques and concepts of physics and biology is avoided, an effort can be made to utilize the general principles developed in the more advanced sciences. In accord with this trend toward increased application of scientific method to psychiatric research, we propose, in the final section, that the important and necessary and now widely recognized emphasis on experimental and statistical controls in the recurring phases of observation constitutes only a partial solution. Experimental and statistical controls will not be so effective without equal emphasis upon "controls" designed to avoid, in the phase of conceptualization, the errors of reification and inflexibility, and particularly the error of unreliability. Reliability is neglected in both the thinking and the research activities of psychiatrists. The contributions of those few who have attempted to institute conceptual "controls" deserve careful study, because the development of methods of increasing conceptual clarity and reliability of terms should not be further neglected, despite the great complexity of the subject matter with which psychiatry attempts to deal.

EXISTENTIAL PSYCHIATRY AS A REACTION TO CONCEPTUAL RIGIDITY

One possible solution to the special conceptual and observational problems which confront the student of human activity is simply to deny the possibility of or necessity for scientific rigor in this area of knowledge. The interest in existential psychiatry seems to represent a current expression of this point of view, which insists upon a necessary and fundamental difference between sciences of nature and sciences of man.

With respect to methods of observation, the advocates of a basically different science of man hold that any attempt to deal with man as an object of study destroys his humanness. With regard to conceptualization, all generalizations, classifications and theoretical systems are viewed by existential psychiatry as inadequate abstractions which fail utterly to capture the unique reality of individual human existence, Procrustean beds into which man's being-in-the-world is forced by the scientist and thereby lost (20).

It would appear that the existentialist movement fills a necessary historical role as a reaction to the rigid misapplication to psychiatry of the observational techniques and conceptual models of the physical sciences. The meaningful study of a human being requires special observational methods. It seems to be true that indifferent questioning of the patient according to some predetermined "mental examination" outline, or hooking him to a variety of recording devices, although useful in getting at certain levels of human activity, is not fully adequate and does not substitute adequately for the sensitively conducted interview in which the interviewer *uses* rather than *attempts to deny* his role as *participant* observer. Apart from the obvious therapeutic demand for participation and relatedness, it seems clear than many aspects of the human individual are available to observation only in a truly transactional setting. The consequent demand upon the therapist-observer for rapid and flexible shifts between the roles of involved participant and objective observer is extremely taxing but need not, in our opinion, lead to the abandonment of efforts to achieve objectivity.

Advocates of the existentialist position in psychiatry argue the futility of theoretical systems and the impossibility of conceptu-

alizing the human experience adequately in any classification schema or body of abstractions (20). The argument is that the whole conceptual phase, which we have described as an integral and necessary aspect of the spiral of science, is an obstacle to the study of man. We agree with Symonds (36), who views the intense interest in existentialism among European psychiatrists as a reaction to the rigidity which seems to characterize psychoanalytic thought there. Shoben (35) sees reification and a conviction of finality about one's concepts as extreme reactions to the problems which surround the search for psychologic knowledge; at the other extreme is a "rigor-is-beside-the-point" position, an existentialist point of view which denies that "generalized knowledge of a testable sort is necessary for effective functioning as a psychologist." Neither extreme is confined to Europe. In the American literature we read that the concepts of psychoanalytic metapsychology represent "the ultimate variables of human behavior" (25)! Interest in the existentialist reaction has become sufficiently general in this country to warrant a new journal devoted to that subject. The stagnant state of a science in which theory has become dogma understandably gives rise to violent attempts to restore movement. There seems, however, to be a real danger that an embryo of hard-won psychologic knowledge as well as possible future advances will be lost in the process. At this point in history it must be admitted that we do not know whether the observing-abstracting-observing spiral we call science can provide the methods for understanding human behavior. It may be that the immediately experienced "knowledge" of the existentialist encounter will be the best we can achieve in this field. At this time our position—favoring a goal which includes abstract, generalized knowledge—is clearly a prejudice, as is our reluctance to agree that the existential approach brings us closer to reality, a reluctance not affected by May's (20, p. 36) claim that existential psychiatry "unites science and ontology." We hold that the principles of scientific method should not be abandoned in the study of human activity without a maximal effort to reduce error in both the observational and conceptualizing phases.

DISCARD OF DIAGNOSTIC TERMS AS A SOLUTION TO CONCEPTUAL PROBLEMS

Theoretical concepts are essential to the scientific method, and it is important that questions be raised frequently regarding choice of the most useful concepts. The papers of Thomas Szasz (37–39) and of Karl Menninger and his associates (22–24) are recent statements of the view that, in psychiatry, diagnostic terms and the disease model are not only useless but are also, in fact, impediments to the advancement of knowledge. To concepts such as "schizophrenia" and "hysteria" they apply Hardin's (12) term, "panchreston," *i.e.*, a concept which purports to explain all and thereby obscures unsolved empirical questions. Szasz and Menninger add that the analogy of behavior disorder to somatic disease is inappropriate, that diagnostic terms are frequently tinged with rigidly held implications for cause or treatment—implications which do not represent established relationships. Moreover, diagnostic terms are often loosely conceptualized and inconsistently used. These authors recommend that psychiatry discard such terms, together with the disease model upon which they are based, or that each term be restricted to use in those certain psychotherapeutic, administrative or medico-legal situations in which it is meaningful.

The history of dissatisfaction with psychiatric "disease" and of the tendency to seek a solution to psychiatry's conceptual problems in the discard of the medical model has been reviewed by Menninger *et al.* (24). Unlike the position of the existential psychiatrists who wish to discard classification and abstraction generally,

this point of view merely suggests the greater value of alternative classification concepts. Both Szasz and Menninger explicitly recognize that there is no science without classification; clearly, their solution is less extreme and more compatible with the empirical philosophy of science than is that of the existential psychiatrists.

Further study of the papers of Szasz and Menninger for specific objections to the concept of mental or psychiatric disease reveals, first of all, a group of objections centering around the assumption that any disease has certain characteristics, that the mere use of the word "disease" carries certain implications. The physician, Menninger (24) points out, is strongly disposed to reify any disease concept, such as "schizophrenia," and then to assume that, as a disease, its cause must lie in the invasion of the individual by some discrete external agent or force, its pathology in some discrete physiologic or psychologic defect or dysfunction, and its cure in some "eradication" of the cause or abnormality by a particular action on the part of the physician. Adoption of this model of *specific* disease narrows the range of variables with which the clinician approaches the patient or a research problem, and blinds him to the possibilities of multiple interacting variables, some of which are influences bearing upon the patient, and others, reactions of his. Szasz (39) is particularly concerned that the disease concept implies that the patient has no responsibility for his "illness" or for participation in its treatment. A well known figure in every psychiatrist's office is the patient who comes anticipating that his neurotic problems will be handled in much the same manner as his recent pneumonia or appendicitis, *i.e.*, with only predominantly passive co-operation required of him. There is also the suggestion, in the model of specific diseases, that the proper conceptual approach is to delineate more and more individual diseases, in the belief that the discovery of their specific causes and cures may thereby be facilitated. Menninger's (24) "unitary concept of mental disease" reflects his conviction that most psychiatric problems are not sufficiently related to discrete variables to make continued or further subdivision a desirable approach.

Szasz (37, 38) sees "disease" as a term which implies the predominant importance of physicochemical causes. He points out that adoption of the disease concept for "problems in living" eliminates a whole area of psychosocial variables which appear to be of considerable importance. Menninger (24) is concerned about the implication of hopelessness which certain disease terms, especially "schizophrenia," have for some psychiatrists who, in consequence and in various subtle ways, lose interest in the patient to whom this diagnosis is applied. In summary, then, the authors whose work we are reviewing describe the following implications which the mental disease concept often has for the psychiatric clinician and investigator: 1) specificity of cause, pathology and cure; 2) lack of patient responsibility; 3) desirability of delineating more diseases; 4) exclusively physicochemical etiology; 5) hopeless prognosis (for some diseases). It is not always clear whether Szasz and Menninger take the position that these ideas are indeed implications of the disease model, or are simply strongly associated with it in medical thinking. The importance of this difference will be made clear in the next paragraph.

All of the very real problems for practice and research created by the adoption of these implications of the disease concept are attributed by Szasz[3] and Menninger to re-

[3] Szasz (37, 38) raises two additional objections to the concept of mental disease. While related to the possible implications of "disease" now under discussion, these additional objections appear to be based more directly upon logical considerations. First, Szasz points out that the inclusion of general paresis, a term based mainly upon physicochemical phenomena, in a list of diseases which includes hysteria, reactive depression and situational maladjustment, is comparable to listing coal, steel and petroleum together with helium, sulfur

tention of the concept. These same problems may also be viewed (and perhaps are viewed by them) as the result of abuse of the concept of mental disease. Further, the abuse is more specifically understood if described as repeated errors of rigidity: 1) The concept of disease *may* be rigidly held to mean a state with specific cause, dysfunction and cure, despite the modern recognition in medicine generally that this limited, nineteenth century concept is no longer adequate for the study even of the infectious diseases (7, 8). 2) The physician *may*, because of a rigidly held conviction that there is a cure by which each disease may be eradicated, convey to the patient that the latter has no responsibility for active participation in the therapy. 3) The research worker *may* focus exclusively on developing more or working with finer subdivisions of disease, and *may* in consequence be blind to the possibilities of broader dimensions needed to establish the important relationships in certain areas of knowledge. 4) "Disease" *may* be interpreted to mean nothing but "organic disease," despite the long countertradition of psychogenic diseases, *i.e.*, diseases at present best conceptualized in the language of psychology and studied by psychologic methods. 5) The psychiatrist *may* rigidly associate schizophrenia and deterioration despite the fact that in 1911 Bleuler demonstrated flexibility in the use of the dementia praecox-schizophrenia concept when he broadened the meaning of the term to include non-deteriorating and even latent cases. The important consequence of viewing these problems as stemming from error in the use of the concept, rather than from the disease concept itself, is that the search for a solution is given a slightly different direction. The desirability of proposing and utilizing alternate concepts is recognized in this modified orientation (as it is by Szasz and Menninger), but the further solution is no longer sought in pressures to discard any concept. Rather, there is now pressure on those who wish to retain certain concepts to avoid error in their use. By this slight shift in focus the errors of rigidity may be reduced without at the same time committing an error of narrowness by discarding concepts before they have had a fair chance to compete for survival, relatively free of error, in the evolutionary jungle of scientific theorizing.

The feasibility of strengthening the disease concept through reduction of conceptual error has received support in both word and deed. Engel (7) appears to believe that, if implications of specificity are avoided,

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and carbon in a periodic table of elements. We suggest that this objection may involve a misunderstanding of the requirements of a classification system or class term. Concepts grouped together in a class (in this case, "diseases") need not share all or even many characteristics, but only the characteristic which defines the class. For "diseases," this characteristic is probably "abnormal or undesirable state of the individual." To insist that paresis and hysteria may not be listed together as diseases because they differ in certain characteristics would be comparable to insisting that helium, sulfur and carbon, though all are "elements" (*i.e.*, share the characteristic of being substances each of which cannot be separated into substances different from itself by ordinary chemical means), may not be listed together because one is a gas and two are solids. Whether the common characteristic shared by all diseases defines a class concept *useful* in psychiatry is of course a quite different question.

Szasz's second logical objection to the concept of mental disease directs attention to the fact that the various disease concepts have limited areas of application. "Hysteria," for example, may be useful for predicting certain relationships in the psychotherapeutic situation, but has little relevance to administrative or medico-legal psychiatry. The opposite may be true for "psychosis." There is no doubt that this is true, but it may be asked whether it is ever otherwise with scientific concepts. As higher order concepts are developed it is hoped that they will encompass more and more phenomena, and thus provide knowledge of wider areas by means of fewer and fewer concepts. Even in the more advanced sciences, however, this process is never complete. To use Szasz's example, for some chemical and engineering purposes it is useful to know the composition of a substance in terms of its component atoms or molecules, but for other purposes a classification system based upon other characteristics (*e.g.*, physical properties, energy content or monetary value) would certainly be more relevant.

the broadened concept of disease may continue to be of value. Benjamin (4) doubts the heuristic wisdom of theories which posit a discrete biologic etiology of schizophrenia, but does not propose to abandon the concept of "schizophrenia." Handlon (11) systematically lists models for the study of the etiology of schizophrenia, including in his list both specific and non-specific conceptualizations, but makes no mention of a need to discard the diagnostic term. The theory of Bellak (2) and that of Rado (26) and Heath (13), both relating to schizophrenia—one a flexible multifactor theory, the other involving a single necessary biologic cause interacting with reverberating cycles of life experiences and adaptive responses—would seem to qualify as examples of a growing ability to shake off the specificity implications formerly associated with "disease."

Finally, to conclude this section and introduce the next, we may note that errors of rigidity are not the only type of error involved in the use of terms designating psychiatric diseases. Both Szasz and Menninger refer to the lack of agreement among psychiatrists in the use of diagnostic terms, *i.e.*, the error of unreliability. "There ought to be a word for this condition," Menninger writes, "which is just the opposite of the Tower of Babel situation. We are not all using different words for the same thing, but we are using the same words for different things" (23, p. 517). Again, however, the question arises as to whether the difficulty is in the disease terms themselves. It would seem to us that all scientific terms are susceptible to this error of unreliability, which is to be avoided not by discarding concepts but rather by efforts to specify clearly their meaning and to improve consistency of usage.

RELIABILITY OF DIAGNOSTIC AND OTHER CLASS TERMS: A NEGLECTED STEP IN PSYCHIATRIC RESEARCH

The literature on the methodology of psychiatric research does give some attention to problems of intersubjective reliability. The papers of Reid (29, 30) and Reid and Finesinger (31, 32) constitute important contributions to this subject. *Definition* is the central theme of the series, and references to the necessarily public nature of scientific terms, to agreement and to reliability are given less central emphasis. It is, however, a matter of two sides of the same coin. "Definition," in its broader sense of the specification and clarification of conceptual meaning, is the principal means by which reliability is to be achieved. Conversely, reliability is the index of the adequacy of definition.

Reid and Finesinger make clear the important influence which new observational findings must have upon existing definitions. There is, however, no contradiction between the need for definitions which change and the most noteworthy point made by Reid and Finesinger—that early definition of terms is necessary to get the spiral going and make meaningful observations possible. They recognize the largely arbitrary and inevitably incomplete nature of meaning given to new concepts in a young science, and suggest that those undertaking conceptualization in psychiatry be workers with extensive experience. They make it very clear that definitions must change to keep pace with new findings, and that many concepts will prove useless, or outlive their usefulness and be discarded. These significant limitations to early definition notwithstanding, Reid maintains the essential importance of assigning some empirical meaning to any concept to be used in empirical science: "Obviously the questions of fact raised by such an hypothesis [about schizophrenia and its relationships to past and future behavior and intrapsychic mechanisms] cannot be answered by any definition of the term 'schizophrenic'; however, be it noted, neither can they be answered *without* adopting *some* definition. In short, ... definitions constitute necessary, but not sufficient conditions of answering empirical questions" (30, p. 400).

Without *some* empirical meaning a concept cannot be related to the observations one makes, the spiral cannot turn, those relationships which will permit a new and more useful (but never final) definition of the concept cannot be discovered.

There have been other statements concerning the importance of reliability to psychiatry. E. B. Wilson's (40) address to the American Psychiatric Association in 1927 is an early example. Colby's (6) *An Introduction to Psychoanalytic Research* lists intersubjective confirmability (which the author equates with objectivity, as we do) among the essential characteristics of scientific research in psychoanalysis. The report of the GAP Committee on Research (9) and the paper of Hamburg and his associates at the Michael Reese Institute (10) are the most forceful recent statements on the subject. Both of these publications make an additional point which requires some discussion. They emphasize that difficulty in achieving reliability should not be permitted to induce the abandonment of important concepts or significant areas of study. There are similarities in this point of view and the position we have taken in response to the suggestion that diagnostic concepts be discarded. It is necessary to add, however, that the indefinite continuation of research programs utilizing inadequately anchored concepts cannot readily be justified unless there are continuing attempts to improve the reliability of those concepts. It is this widespread practice, not of abandoning the research area or concepts, but rather of neglecting or abandoning efforts to achieve agreement, that has led to the current situation, in which almost any psychiatric concept, diagnostic or other, may justifiably be labeled "panchreston" on the single basis of lack of adequate empirical anchoring. Unconfirmed postulates of relationships, fundamentally unconfirmable because their terms are relatively meaningless, are accepted because they are plausible and provided by respected authorities, and are fed back into revised definitions of concepts in a *pseudo*-empirical analysis which leads not to an advancing spiral of more and more powerful higher-order concepts, but to a downward spiral of less and less meaningful concepts and postulates.

Enormous amounts of time and energy are required to increase the objectivity of most of the concepts which are of interest to psychiatry. This makes research in psychiatry in many ways much more difficult and demanding than in those areas of medicine chiefly dependent upon concepts measurable with the techniques of the biologic and physical sciences. But neglect of this problem will not eliminate it. To the extent that the worker considers it necessary or desirable to employ complex, difficult-to-objectify concepts, he should perhaps be prepared for years of preliminary definition and conceptual clarification and training of colleagues in the application of these strengthened concepts.

For some purposes it is possible to choose simpler and more easily objectified concepts, but much psychiatric research seems to demand concepts of great complexity in order to do justice to the complexities of our subject matter. This latter requirement Schwab (34) has called the demand for validity. The psychiatric investigator is frequently confronted with what this author has depicted as the reliability-validity dilemma: "On the one hand, the scientist is responsible for making conclusions which are as clear, dependable and precise as the state of his art permits. On the other hand, he is responsible for seeing to it that the subjects and predicates of these conclusions ... be as 'telling' as possible, that they represent as much of the extent and complexity of the subject as the state of his art permits." The authors of the present paper are among those described in a later paragraph by Schwab as avowing both reliability and validity as necessary and desirable in science but affirming the necessary primacy of one. In our case, that one is reliability. Reliability does not insure that a concept will be useful (valid) for furthering knowledge;

but no matter how valid or "telling" a concept may seem, its contribution to empirical science will be no greater than its objectivity. In this sense, reliability becomes the more fundamental consideration.

Diagnostic concepts are among the variables most frequently employed in psychiatric research. These variables are among the more complex which do not readily and automatically lend themselves to reliable application. Ash (1), for example, reports: "Agreement between two psychiatrists in diagnosis even as high as that approaching a fifty-fifty split of opinion was found only when the diagnoses were grouped into very broad categories. Even in these terms three psychiatrists agreed in less than half the cases, and no pair agreed in more than about two-thirds of the cases." According to Mehlman (21), individual psychiatrists, each confronted with patients non-selectively assigned from a large sample, used different diagnostic categories with significantly different frequencies. In the study of Hunt, Wittson and Hunt (16), agreement between two psychiatrists on a group of 794 patients as to broad diagnostic categories (psychosis, psychoneurosis, psychopathic personality) was 54 per cent, while for any more specific diagnosis, agreement was 33 per cent. Raines and Rohrer (27) found that a "group of psychiatrists showed significant differences in the frequency with which they observed different personality types in random samples drawn from the 886 men and... different psychiatrists tended to see different personality traits in the same man." In a more recent study (28) these authors report evidence which they believe confirms their hypothesis that these differences are related to the personality of the psychiatrist. Finally, Schmidt and Fonda (33) report satisfactory reliability of psychiatric diagnosis only at the major points of differentiation, *e.g.*, organic psychosis vs. schizophrenia.

Despite this evidence of poor reliability, and despite the frequency with which diagnostic or other complex behavioral variables are used in research, there has been little interest in reliability on the part of psychiatric investigators. In an attempt to determine just how widespread a neglect of reliability exists in current psychiatric research, the present authors surveyed 100 consecutive papers appearing in five widely read psychiatric journals[4] between January and August, 1960. The only criterion for selection was that the authors of these papers purported to demonstrate the presence or absence of a relationship between two variables, at least one of which was behaviorally defined. Seventy-three of the 100 papers contained no mention of reliability, yet represented attempts to correlate variables, at least one of which was so complex that the question of agreement is legitimately raised. An example of this common disregard for reliability is the attempt to relate undefined "clinical improvement" to a particular treatment. Clearly, such variables as "clinical improvement," "degree of psychopathology" or "schizophrenia" are so complex and so loosely defined that measurement of agreement in usage of such a term might usefully precede any attempt to correlate it with other variables if meaningful conclusions are to be derived.[5]

In only 19 studies was the reliability of each major variable assured, either by direct assessment of agreement by the authors or previous workers, or by employing less complex variables which could be accurately measured, *e.g.*, speed of tapping on a tap board, with automatic recording of number of taps per minute. It is of course understood that all measurements are ap-

[4] American Journal of Psychiatry, A.M.A Archives of General Psychiatry, Journal of Nervous and Mental Disease, Psychiatry, Psychosomatic Medicine.

[5] It is perhaps less apparent that some of the more easily observed variables, such as the blood pressure response following the injection of epinephrine and Mecholyl, might be unreliable; yet Blumberg (5) has shown that this test is not reproducible on serial testing when the conventional methods of classification are used.

proximations, and that perfect reliability is unobtainable. It is desirable, however, to know the amount of measurement error in a particular variable, so that either the reliability may be improved or the variable discarded.

The eight remaining papers made some mention of reliability of the major variables, one actually assessed the reliability of one variable but did not demonstrate the reliability of each relevant variable.

The GAP committee (9, pp. 600–601) has indicated that logically consistent, unambiguously specified meanings are a primary requirement for the achievement of reliability in the use of terms. It is important that both the more arbitrary elements of the "definition" and those elements which are considered to represent empirically established relationships be included in statements of meaning. In addition to clear specification of meaning, attention must be given to the communication of this meaning to those involved in the research. The paper of Hamburg *et al.* (10) makes it clear that a training program consisting of repeated group discussions of ratings, with efforts to obtain consensus, was a necessary condition for the successful achievement of agreement. In more general discussions of obtaining reliability, Mandler and Kessen (19, p. 62 ff.) and Heyns and Lippitt (14, pp. 396–397) emphasize the importance of training for agreement through continuous review of definition and discussion of reasons for failures to agree.

The present authors are currently engaged in the preliminary reliability-establishing phases of a research program based upon the prediction that complex personality classification variables assessed in the unique setting of the clinical interview are likely to prove valuable for many problems of interest to psychiatry. The classification concepts of the Leary (18) interpersonal system of diagnosis were chosen for this work both because they seemed to possess heuristic value and because they appeared to be well anchored empirically. Some adaptations of these variables which seemed necessary for their use in the interview (as contrasted with their use based on formal psychologic tests) were made and incorporated, after preliminary trials, into a manual for interviewer-raters. A patient is seen independently by pairs of raters who subsequently make independent diagnostic judgments. Each pair of raters then meets to discuss sources of disagreement and to attempt to reach agreement, although for purposes of measuring reliability, of course, only their original independent ratings are used. The entire research group meets periodically to discuss systematically recurring sources of disagreement and to approve clarifications of meaning, which are circulated in writing to all participants and eventually incorporated into revisions of the raters' manual.

We wish to emphasize the frequent memoranda and revisions of the manual because we recognize that, insofar as the training is written, there is no major problem of its subsequent transmission to other workers in, say, another city. There is, however, in any program such as the one described an implicit sharing of meanings through verbal and non-verbal, but unwritten, communication. This implicitly shared element in the use of concepts by a research group is not mentioned to suggest its elimination, since it seems unlikely that the training of coworkers can take place entirely through printed instructions or prepared lectures. Rather, the suggestion is that it be recognized, and not permitted to become so large an element in the training for reliability that only those working in a given "center" or in very direct line of personal contact with its members can reproduce its findings. These considerations in no way affect the more general point that conceptual clarification and training are essential in the establishment of reliable use of complex behavioral concepts.

The proposal that the achievement of

reliability is an important and necessary step in psychiatric research is not new. We refer again to Wilson's (40) 1927 academic lecture to the American Psychiatric Association, in which he said: "The point to be emphasized is that there is scientific fact independent of the observer only insofar as there is consensus, and a nearby step is to improve definitions and the technique of measuring or classifying so that there shall be closer agreement." There is still need for increased concern about this procedural step in psychiatric research. It is possible that both the need and its implementation may now be more clearly viewed within the perspective of modern scientific philosophy.

SUMMARY

Some recent trends in psychiatric thinking represent efforts to solve the conceptual problems which we view as the result of excessive rigidity in the use of language or excessive looseness. Three such trends are discussed: 1) the suggestion of existential psychiatrists that abstract concepts be avoided; 2) the less extreme suggestion that the disease model and the specific concepts of psychiatric disease are useless and even harmful to psychiatric progress and should be discarded; 3) the suggestion that, in addition to control of the observational aspects of psychiatric research, the conceptual aspects should also be "controlled," through measurement of the intersubjective reliability with which terms are used, and through efforts to improve that reliability by means of definition and clarification of terms and the communication of the clarified meanings to all participating in the research.

The first—or existential—trend is considered to be an extreme reaction to the rigidity which does characterize much psychiatric theorizing. Abandoning classification, abstraction and theory is an extreme solution, because it would mean abandoning the scientific study of human activity. Until a scientific methodology more nearly free of major errors of conceptualization has been given a trial, its abandonment is not warranted.

A number of criticisms of mental disease concepts have been raised. In particular, the inclusion of specificity in the concept of disease has unfortunate implications when that concept is used in psychiatry. However, certain aspects of the disease model may be useful in a clinical science. Also, there may be elements of heuristic value in certain of the specific disease concepts, such as "schizophrenia." We therefore offer the suggestion that such concepts be freed of errors of rigidity and unreliability, and accepted for use along with any alternative concepts which seem to offer promise—each research group being free to select according to its judgment. It is our conviction that, if rigidity is avoided, those concepts of lesser value will perish in a process of natural selection.

Finally, we feel that, despite a few excellent recent attempts to call attention to the importance of reliability of terms and the related needs for definition, conceptual clarification and explicit sharing of meanings, there is still insufficient recognition of this essential step in psychiatric research.

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FREUD'S THEORY OF PSYCHOSIS AND THE ROLE OF THE PSYCHOTHERAPIST[1]

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The conceptual tools provided by Freud's psychoanalysis enabled a band of psychiatric pioneers to begin work with a new kind of patient. This patient, previously considered to be incomprehensible and hence inaccessible, was called the schizophrenic.

In large measure, Freud's basic contribution was to make possible communication with the schizophrenic, or as Knight (21) has said, to tune in on his primary process wave length. For Freud's insights into the primary process and his clarification of the psychology of dreams were as important for what was to follow as were his specific studies of psychosis itself.

At first, armed with new ideas and new self-confidence, psychiatrists were able to sustain prolonged, intimate therapeutic contact with schizophrenic patients. But the patients themselves had an even greater impact. The fresh experiences, highly charged with emotion, assumed tremendous significance for the doctors who encountered them. Work with schizophrenics passed the point of being a technical approach to a particular problem: in some instances it became the basis of a whole new point of view about psychotherapy, and even a new psychology, a new point of view about the behavior of all human beings.

No one can deny the extent to which psychotherapeutic theory and technique have been enriched by the leaders in this area. At the same time, intensive contact with and therapeutic responsibility for schizophrenic patients occasionally seem to remove the psychiatrist from the realm of hardheaded observation to that of personal philosophy and self-perception. Indeed, psychotherapists' preoccupations with themselves occupy a significant percentage of the literature in the field. This is not surprising. The schizophrenic patient does challenge the therapist to work miracles. He cannot be helped without long and arduous personal involvement. Savage (25) and others have noted that schizophrenics have violent passions and produce violent reactions, that by their tempestuous behavior they disturb the therapist's private life, that they work hard to make the transference real, and that they are often successful in converting the therapist into a rejecting parent. There may also be elements in the schizophrenic personality which appeal to psychiatrists, who themselves are concerned with symbolic representation, sensitivity to non-verbal communication, and the use of primary process forces in intellectual and artistic creation.

Certainly the occasional "cure" which may occur without apparent reason contributes to a tendency to construct highly personal theories about the illness and its treatment. Also, there is some evidence that certain therapists, by virtue of the kinds of people they are, have more success than others with schizophrenic patients.

In this situation it seems particularly important to delineate an internally consistent theory of the psychodynamics of the disorder being treated. Many psychologic theories of schizophrenia have been advanced; it is probable that all, in one guise or another, contain Freud's fundamental propositions about the nature of psychosis. This historic factor may con-

[1] This paper is based on material presented at the Forest Hospital, Des Plaines, Illinois, October 26, 1960, and The Langley Porter Neuropsychiatric Institute, University of California School of Medicine, San Francisco, January 18, 1961.

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ribute to the current difficulty in defining specific, psychologically useful concept f schizophrenia, in contrast to a general oncept of psychosis.

DEVELOPMENT OF FREUD'S THEORY OF PSYCHOSIS

Freud's initial speculations about the psychodynamics of psychosis represented a direct extension of his early formulation of hysteria: psychotic behavior reflects a defense against an unbearable idea and the anxiety or guilt associated with it. In 1894 the unbearable idea was described (10) as one associated with some aspect of current reality, so that the psychosis could be considered a "flight" from a disturbing life situation via the development of wish-fulfilling delusions and hallucinations. The thesis was illustrated by the case of a young woman who suffered a traumatic event—a disappointment in love—followed by a state resembling mild elation: she hallucinated her lover's presence and "lived for two months in a happy dream." Interference with the defensive behavior pattern resulted in disorganization: "... She was happy as long as she was left undisturbed and only raved when some circumstances of her surrounding prevented her from carrying out the logical promptings of her blissful dream."

This reaction may be regarded as a "hysterical psychosis," the symptoms reflecting a simultaneous defensive and wish-fulfilling activity. Denial and hallucination dealt with the patient's anxiety and sense of loss as well as with her probable hostility against the disappointing lover. Her symptoms also posed an effective barrier between her and any real human being who might replace the lost object.

Freud did not elaborate upon the concept of psychosis, inherent in the description, as a waking dream. The implication is clear, however, that psychotic and dream thought share, at least, a wish-fulfilling or need-gratifying quality, and that neither is limited by ordinary rational considerations.[3]

From the viewpoint of therapy, this account offers a hint of the difficulties met by the doctor wishing to penetrate the waking dream and snatch the patient back to reality. In recent years it has been suggested that the therapist participate to some degree in the structure of the psychosis. His job is not to effect a frontal disorganization of the patient's private view of the world, but to provide a relationship which will make it possible for him to abandon his psychotic gratifications.

By 1896, Freud was less concerned with the immediate situation itself than with its significance as determined by past events. Early life experience was now conceived as evoking feelings of "self-reproach." Under certain circumstances these feelings, with their associated memories, emerged into consciousness disguised as unrelated obsessional fears. If the repressive defense failed completely, the reproach might return in "its original form," possibly with the intensity and significance of current experience. At this point the self-distrust of the obsessional is changed into distrust directed against others, through a process which Freud labeled "projection." Additionally, a change in the self-evaluating functions of the ego was postulated: the return of the repressed "absorbs the thought-processes of the ego until they finally become accepted without contradiction" (11).

Here are the indications for a therapy aimed at conflict resolution and genetic clarification, as well as for the approach directed to ego functions. These ideas are relevant to the treatment of both neurosis

[3] C. G. Jung and Eugen Bleuler, of course, wrote at length of this aspect of schizophrenia. Bleuler noted the "symbolisms, condensations," etc., common to dreaming and schizophrenic thought, and stated that "it may yet be possible to show that the secondary symptomatology of schizophrenia is wholly identical with that of dreams" (Bleuler, E. *Dementia Praecox or the Group of Schizophrenias*, p. 440. Translated by Joseph Zinkin, International Universities Press, New York, 1950).

and psychosis, although they contain the suggestion that the therapist might have to rebuild the schizophrenic's repressive defenses.

The concept of psychosis presented here is more complex and less well defined than that of the earlier "hysterical" variety. A predisposition to illness lies in the feeling of "self-reproach" associated with crucial early experiences; but there is nothing here of pathognomonic specificity for psychosis as contrasted to neurosis, or for schizophrenia as a particular type of psychotic reaction. At the cross-sectional level, however, a failure in repression as a response to current stress is identified as basic to psychotic behavior in two ways: 1) it permits the vivid eruption into consciousness of material ordinarily wholly or partially out of awareness; 2) it modifies the various ego functions concerned with evaluating reality. Under these circumstances the existence of a mechanism specific to a particular aspect of psychotic behavior, *i.e.*, "projection," might be inferred.

In 1911, in his analysis of Schreber's memoirs (12), Freud came back to his defensive scheme, but now he focused upon the patient's *internal* drives. The defense reflected in psychotic paranoid behavior was considered to be directed against a wish, not an external event; and the wish was identified as the desire for passive homosexual gratification. Formulae explaining the various types of paranoid delusions utilized the concepts of withdrawal of libido from the loved person, denial of the homosexual element in the love which has been withdrawn, and its conversion into the opposite, hatred. The feeling of hatred is then projected and rationalized, *i.e.*, justifying evidence is adduced and made congruent with the person's other perceptions. This helps to crystallize it into a logic-tight system. Projection brings the libido back again to the people it had abandoned. Thus, a delusion concerning the interest of other people in the patient reflects an attempt at psychologic recovery, a re-establishment of his connections with the world around him.

This formulation represents a major step toward specificity in Freud's concept of paranoid behavior. Although it does not contain a theory of schizophrenia *per se*, the concept of libidinal withdrawal has general applicability to psychotic states. It is significant also insofar as it systematically relates the intrapsychic and the interpersonal process. The human being is defined in these terms as a system with an innate tendency to relate to similar systems, a tendency which is operant even in the face of severe disorganization and which may be reflected in a variety of behaviors.

HUMAN RELATEDNESS AND THE SCHIZOPHRENIC PERSON

The patient's problem in object relations or, in Sullivanian terms, interpersonal relations—for Freud's objects included people—has been considered by many as the fundamental difficulty to be approached in therapy. Freud's formulae clearly imply that a direct attack on delusional content, apart from its meaning in terms of therapist and patient, will probably be unproductive. The rationalizations or justifications for an attitude (for example, "*I hate him—because he persecutes me*") contain the heart of the delusional idea. They explain everything, and bring many chaotic feelings and perceptions into focus; and the known can be dealt with more easily than the unknown. The paranoid system provides the patient with a basis for action. Freedom to be self-righteously hostile to one's supposed enemies is important in the maintenance of personal security, and allows aggressive tension to be discharged in fantasy if not in action.

The importance for therapy of any patient's way of relating to others is inherent in Freud's writings. Freud noted in particular that the therapeutic situation elicits in

intense form the expression of latent feelings and character traits: "Peculiarities... which he had seemed to possess only to a modest degree are often brought to light in surprisingly increased intensity, or attitudes reveal themselves... which had not been betrayed in other relations of life" (13). This insight has been the basis of many current psychotherapeutic efforts which involve interpreting or responding to transference behavior. It has also made possible the recognition of latent psychotic behavior which may be masked by a rigid conventional façade (4).

A particular aspect of an individual's way of relating to others is suggested by the concept of narcissism. The narcissism of the schizophrenic was noted in the Schreber report. A narcissistic relationship also appears to be the one most readily available to him. In terms of this theory, the important person, the one who may provide the patient's bridge to reality, should share certain personality attributes with him. Perhaps such a consideration was behind Sullivan's thought that a schizoid attendant might be the hospitalized schizophrenic's most effective communicant (30). This concept of narcissism contributes to a view of the schizophrenic as one who, in adulthood, may perceive the world in terms similar to those of the simultaneously helpless and omnipotent infant. It also makes it possible to view schizophrenia *qua* illness in relation to schizophrenia as a "way of life" attendant upon certain character structure. Both considerations emphasize the special significance of communication for the psychotherapist of schizophrenia.

My own observations have led me to believe that the first step in therapy does involve an unwitting communication from doctor to patient. The patient learns *about* rather than *from* the therapist, and what he learns must be something rather specific —namely, that he and his doctor have some community of experience. The patient's discovery in this respect makes of the doctor a more suitable object for identification, *i.e.*, it facilitates the patient's acquisition of some of the doctor's ways of behaving (7).

Milton Wexler regarded the major task of the therapist of schizophrenics to be the replacement of a punitive superego, based upon introjection of the mother, by a constant "mental image" of himself, and proposed to achieve this by agreeing with his patient's self-condemnations (32). Bak considered Wexler's most significant action, however, to be the element of agreement *per se*, making it possible for him to represent a narcissistic object, *i.e.*, one appearing to the patient as part of himself (1).

Here is the point where psychoanalytic theorizing begins to overlap with a fundamental common sense. John Reid has pointed out that *all* men feel rational and self-confident insofar as they meet with agreement—which, according to Santayana, is a "form of friendship" (24). The emphasis on agreement, similarity and shared experience, however, which appears to be important for the initiation of therapy with schizophrenics, has not been considered to be of equal value for work with neurotics. In this instance the maneuvers of the therapist must be a function of the degree of the patient's regression, the structure of his character, and his means of adaptation—the ways in which he tries to maintain some stable reciprocal relationship with his environment.

Another element which may be specific to the theory of psychotherapy of paranoid schizophrenics is the role of passivity and masochism. Conflicts involving passive-masochistic wishes are suggested by the discussion of Schreber's illness as important determinants of the schizophrenic's way of relating. Schreber's fantasies of being a woman "submitting to the act of copulation" followed his disappointment at being unable to have children, and he later implied that transformation into a female

would allow him to produce a new race of men as a result of direct impregnation by God.

Experience with a number of schizophrenic men has suggested that many of their delusional ideas are the ultimate elaboration of a wish for union with their mothers in which they would lose their shaky existence as independent masculine entities (6). The earliest phase of the process of fantasied unification can be considered to be similar to the defense of identification with the aggressor: the child himself becomes the powerful adult and avoids the threatened danger. When, as in Schreber's case, the father is actually powerful, sadistic and suppressive (23), union by identification or by submission in the masochistic feminine role appears to be the only available solution. The patient who asserts that he is influenced by others is announcing both his connection with the symbolically significant person or group in question, and his relinquishment of certain executive ego functions to them. In effect, he abdicates his individual responsibility and becomes a part of some larger body.

This is again compatible with the idea that the successful therapist is one who offers himself, wittingly or unwittingly, as an object of identification, a person with whom the patient can for a time experience a sense of connection. The associated problem, of course, is that of dealing with the fears of being overwhelmed or engulfed which arise as the countercurrent to the wish to lose one's identity in that of a parental figure.

INTERNAL CONTROLS, MASOCHISM AND HOMOSEXUALITY

The mainstream of Freudian thought was continued in Tausk's classic paper of 1919 on "the influencing machine" (31). This contained the first systematic interpretation of a single symbolic element in a psychotic delusion. Tausk also utilized the concept of ego-boundary, conceiving the influencing machine as a projection of the patient's body. The patient who has lost his ego boundaries is subject to control from without and, like the child before he has told his first successful lie, can have no secrets from anyone. Similarly, Tausk regarded catatonia as corresponding to the stage when an infant is still discovering his body part by part, as though it were in the outer world and subject to domination by outside force.

The establishment of an internal control system based on the introjected parent had been noted in Freud's paper of 1914 on narcissism. Here he advanced the hypothesis that delusions of being watched or observed reflect the activity of the conscience in its regressive form. Conscience, thought to originate from the influence of parental criticism, was defined as the psychic institution which watches the ego and measures it by the ego-ideal (14). This concept is basic to Wexler's idea, noted above, that therapy must involve the replacement of a pathologic superego by an introjected image of the therapist. Another statement of this idea might refer to the learning or acquisition of the therapist's standards, or to identification with him rather than to "introjection" of his "image."

In the following year, reporting the case of a woman who developed a delusion of having been photographed in her lover's bedroom, Freud stated: "Love for the mother becomes the protagonist of all those tendencies which, acting as her 'conscience' would arrest the girl's first step along the new road to normal sexual satisfaction. . . ."

Here was emphasized not only the significance of introjection, and the patient's homosexual concern with her mother, but also the interpersonal significance of her symptom. The paranoid delusion effectively interrupted her developing heterosexual affair, which had competed with her love for her mother (15).

A related theme was developed in "A Child Is Being Beaten," published in 1919. This paper contains the idea that people with masochistic beating fantasies become especially sensitive toward father-figures, are easily offended by them, and in that way make the imagined situation come true. "I should not be surprised," Freud wrote, "if it were one day possible to prove that the same fantasy is the basis of the delusional litigiousness of paranoia" (16). This suggests that the fantasy of being beaten may occur in place of a delusion of persecution, and that either may defend against as well as gratify an unconscious passive erotic wish. In interpersonal terms, the litigious behavior unconsciously designed to provoke a beating from a symbolic father actually serves to isolate the patient from others and thus to aid in his defensive process.

Again, this is an example of the type of theoretical formulation which may contribute to the psychotherapist's awareness of his role *vis-à-vis* the patient, and will determine the nature of his intervention or silence. He must predict to himself the consequence of gratifying the patient's wish through aggressive behavior, or of reinforcing his expectation of punishment; or he must gauge the possible impact of silence as rejection, or as supporting the patient's distance maneuver. In each instance, the technical move is grounded in an internally consistent theory.

Freud's hypothesis about the possible relationship of beating fantasies to paranoia was elaborated upon by Bak. Bak concluded that delusions of being mistreated, injured and persecuted gratify the original masochistic desire to be castrated, beaten and abused by the father. Thus, the paranoid delusion becomes a return of the repressed, and paranoia is delusional masochism (2).

An illustration of the role of passive wishes in the genesis of a paranoid psychosis is provided in Ruth Mack Brunswick's follow-up study of Freud's "Wolf-man" (8). Driven by concern about a fancied lesion on his nose, the patient consulted a series of physicians, father-figures toward whom he had strong anxiety-laden passive wishes. Indications of their affection or indifference resulted in elation or paranoid despair. The final psychotic response, according to Brunswick, stemmed from the Wolf-man's unresolved transference to Freud. His passive homosexual trend was apparently reinforced by the latter's regular financial help, which was perceived as evidence of interest beyond that usually displayed by a physician for his patient. A therapist's sympathetic, supportive behavior may thus contribute to a patient's difficulties. Brunswick traced the patient's reduction of anxiety about his passive wishes during treatment with her to the point where he was able to give them up completely. She suggested that her being female facilitated treatment by avoiding the development of an intense homosexual transference.

HOSTILITY AND HOMOSEXUALITY

The paranoid patient's expectations of other people were again the subject of Freud's attention in 1922. At this time he suggested that their awareness of ordinarily subliminal cues in the behavior of others provides a reality basis for delusion formation. On the one hand, "... the enmity which the persecuted paranoiac sees in others is the reflection of his own hostile impulses against them"; on the other hand, he "expects from every stranger something like love" and "is not far wrong" in regarding their indifference as hate in comparison with his needs (17).

This formulation contains the idea that the paranoid person has such strong needs for love that he expects and finds rejection from those about him. It is a thesis compatible with the earlier emphasis on pas-

sivity and masochism, and is basic to many current approaches which emphasize the patient's sensitivity to unverbalized cues in the therapist's behavior, particularly those which may have hostile significance.

Destructive wishes were first granted primary significance, in contrast to their secondary defensive role, by Freud in 1923. He proposed that the dangerous rise of hostile feelings in the sibling-rivalry situation may be dealt with by the mobilization of their opposite—feelings of love—or by identification with the hated object. The potential paranoid was regarded as one with an ambivalent attitude toward people, and at the time of the actual psychotic development, energy was said to be "withdrawn from the erotic impulses and used to supplement the hostile energy" (18).

When it is not possible to mobilize feelings of love, or when such feelings carry the danger of homosexuality with them, the more primitive defensive operation of identification may become necessary. It might be logically concluded that psychotic behavior can reflect a regressive identification, a defensive merging in fantasy with some figure symbolic of parents or siblings. Bak has suggested a defensive regression of the ego to its undifferentiated phase as the schizophrenic's method of avoiding destruction of his object world. In this phase, the fusion between self and objects eliminates the operation of aggression (3). Perhaps the catatonic patient, completely occupied with paranoid fantasies, may be understood in this fashion. His fantasied membership in what Cameron has called the "paranoid pseudo-community" (9) is supplemented by muscular immobilization, which not only inhibits the expression of hostile feelings but also enforces his lack of actual contact with people who might provoke frightening, destructive impulses in him.

Freud's position was extended in 1940 by Knight, who emphasized the *need* for love, as much as the loving attitude, as reflecting the paranoid's effort to deal with unconscious hostile wishes. Love is dangerous insofar as it stems from passive sexual wishes directed to the father, who is hated because he prefers mother and frustrates the son's passive wishes. As the future patient grows up, he attempts to love and be loved by father or father-surrogates as an aid in repressing his hostile feelings and simultaneously as a way of enticing his father to his destruction. "It is the preponderance of the hostile, seductive motive that makes his homosexual love for his father so dangerous and prevents him from really developing into an overt homosexual" (22). The patient cannot allow himself to get too close to father because he runs the risk of destroying him. This argument suggests a similar way of regarding the patient's problem in achieving some closeness with his psychotherapist.

REALITY AND THE CONTEXT OF PSYCHOSIS

In 1924 Freud restated the psychotic's problem in dealing with reality. Neurosis, he said, tries to ignore reality. Psychosis denies it and tries to substitute something else for it. The distressing character of many delusions and the anxiety associated with them indicate, however, that the process of remodeling reality is conducted in the face of strenuous opposition: that is, the rejected part of reality re-asserts its claim, as does the repressed instinct in neurosis (19).

This formulation recognized that denial of external reality is never completely successful (something which today is familiar to all psychotherapists of schizophrenics), and that psychotic behavior incorporates significant elements of the real world. Further, the concept of delusional or other psychotic behavior as embodying some disguised gratification of a potentially disturbing wish implies that the "opposition," as Freud termed it, must include all of those factors which deter unacceptable wishes, thoughts and feelings from emerg-

ing into consciousness. Thus, the production of paranoid ideas or psychotic action patterns proceeds against dual resistances: one is the force of repression which, while less important than in the neuroses, still excludes certain data from consciousness; the other is the intrusive significance of the external world, which continues to be a determinant of behavior.

The indications for the therapist are clear. His interventions cannot be based solely on his evaluation of the patient's historically evolved needs. The most bizarre behavior must still be viewed in part as a function of the context in which it occurs. This concept seems basic to the later view, implied by Sullivan and articulated by Stanton and Schwartz, of schizophrenic behavior as a mode of participation in a social field (27). Their concept of a symptom as stable only insofar as it is part of a stable social equilibrium (29) is fundamental to any intelligent understanding of milieu therapy. The social field, in which the patient's activities are responses and adaptations, may be "split" by covert conflict between staff members upon whom a patient is dependent. In that event, the schizophrenic patient's excitement or dissociation is considered to be a "reflection of, and a mode of participation in" the split social field (28).

Other observations indicate that a major constituent of the social field may be a key person upon whom the patient is more dependent than he is upon others. In this instance, the integrative and executive ego functions of the chronic schizophrenic existing in a limited hospital environment may fluctuate in scope and effectiveness with the relative intensity of gratification and deprivation from the key person upon whom he is most dependent for libidinal supplies (5). This seems to be especially true in a large, anonymous situation, with many personnel coming and going in an unpredictable way.

Otto Will has recently suggested the value of small hospital units, where everything can be made predictable and the patient can know every member of the staff. This might reduce his feeling of powerlessness by giving him some real control over his environment. Presumably, such a milieu would help to promote a clear differentiation for the patient between himself and others (33).

Freud's final statement on the problem (1937) included a specific suggestion for therapy. He speculated that the fixed character of delusional ideas may be a consequence of the fact that they contain a fragment of historical truth. He then wondered if recognition of the historical kernel of truth might not afford common ground upon which the therapeutic process would develop. That process would consist in liberating the fragment of truth from its distortions and its attachments to the present day, and in leading it back to the point in the past to which it belongs (20).

The doctor who can recognize the valid basis of some of the schizophrenic patient's beliefs becomes a very important person for that patient. Mutual agreement as to the significance of an idea or a feeling can initiate a relationship which will permit the later questioning of symptomatic ways of behaving.

Freud also recognized the importance of immediate reality in another therapeutically relevant way, that is, his concept of the significant dynamic process of delusion formation: "... The turning away from reality is exploited by the upward drive of the repressed in order to force its subject matter into consciousness...." (20).

This idea may be restated: the operation of denial, as a mechanism of defense, decreases the effectiveness of repression. It implies that a continuous contact with external events is necessary for the maintenance of the repressive defense. From a slightly different viewpoint, it suggests that a continuous flow of information about external events is required for the continuing

correction, ordering and evaluation of internal events. The therapist's function, in these terms, includes giving the patient as much stimulation, as much usable information as possible. Sometimes this requires much talking, such as clarification of events in the hospital, and letting the patient know who's who; it may even include the therapist's soliloquies about himself; but sometimes it may be sufficient (and all that the patient can tolerate) to have the therapist come, regularly and predictably, for his appointments.

COMMUNICATION AND INTERPERSONAL CONTACT

It is clear, on looking back, that Freud's various allusions to the importance of the therapist, his way of relating, and the patient's ways of perceiving and relating, all converge upon each other. Perhaps we can agree on the significance for patient and doctor of a moment of true communication, of contact, with each other. Such a moment can be considered to be a point in time when the therapist's existence is shared with the heretofore isolated patient. It may at least represent a significant intrusion of the therapist into the patient's private world. And yet the intrusion must be limited. It is surely important to identify those tendencies in the therapist which can unwittingly perpetuate the psychotic process insofar as they help to re-create for the patient his parents' vicarious use of him to gratify their own needs or resolve their own conflicts. With the schizophrenic, as well as with other people, we continue to be concerned with permitting development as well as producing change. Our most optimistic view sees nascent qualities awaiting the opportunity to become alive, effective and organized.

Paraphrasing Sonneman, we may say that the therapist must have an unconditional readiness to recognize the other person, and to share his world with him. Without such openness of his inner horizon he may "turn the other into an object within (his private world), a shadow of (his own) being" (26).

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CONCEPTUAL CONFUSION IN EATING DISORDERS[1]

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The close relationship between emotions and eating habits is old medical as well as popular knowledge. Loss of appetite is generally recognized as an expression of grief, depression, and mental and physical illness. Conversely, starvation produces loss of weight and physical decline, together with marked psychologic disturbances (2).

The fact that pronounced changes of body weight can develop as consequences of eating disorders due to psychologic causes is more recent knowledge, though the psychologic origins of cachexia were recognized in 1689 by Morton (21). He described the condition as "nervous consumption," carefully differentiating it from tuberculosis. The condition was discovered again and delineated independently in 1873 by Lasègue (16) and Sir William Gull (12), and is now known in the English literature as anorexia nervosa.

Recognition of emotional factors in the more common condition of obesity came much later. Occasional references are found in the literature of the nineteenth century (17, 19), but they became more numerous only after the First World War, when Lichtwitz (18), in Germany, noted that during periods of grief and uncertainty some women tended to put on weight. This phenomenon was described as "paradoxical obesity" during the Second World War by Dreyfus in France (9).

The first American studies were reported from the fields of medicine (23, 24) and pediatrics (1). In studies of childhood obesity the relationship of early experiences and disturbances in the family interaction was recognized (7).

Psychoanalytic investigation increased the understanding of the psychodynamic and symbolic significance of disturbed eating habits. The intricate ways in which human factors impinge upon all biologic functions and needs were recognized by Freud. The gamut of emotional development, character and symptom formation has been traced to the interaction among instinctual demands, the more fortunate or hapless ways in which they are met, and to the ways an individual reacts to them.

Freud (11) proposed many theoretical formulations for this fundamental concept of his teaching, but remained critical of all his efforts to formulate a fixed theory of instincts. He defined instinct as "a borderland concept between the mental and the physical, being both the mental representative of the stimuli emanating from within the organism and penetrating to the mind, and at the same time a measure of the demand made upon the energy of the latter in consequence of its connection with the body." This definition may be applied to any bodily need, and throughout his work Freud maintained that there was more than one instinct. Yet his preoccupation was with the sexual drive, and disturbances in the nutritional field were considered to be secondary to disorder in the psychosexual development, or a symbolic expression of disturbances in other areas of living.

The psychoanalytic approach not only deepened the understanding of the problem but also offered new therapeutic possibilities. Obese patients seemed to be particularly gifted in grasping psychodynamic relationships and in acquiring an extensive psychoanalytic vocabulary. Yet not infrequently, after what at first appeared to be

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a dramatic success, a sudden relapse occurred and it became apparent that the basic problems had remained unaffected. Meyer (20) concluded a brilliant discussion of the psychodynamics of anorexia nervosa and bulimia by stating: "With regard to treatment, I must confess pessimism." Nemiah (22), reviewing anorexia nervosa, is equally skeptical of the value of insight-giving psychotherapy.

My own observations also led to the conclusion that the traditional interpretative psychoanalytic approach was in need of modification (5). This applied in particular to patients suffering from developmental obesity (3), which in many aspects is related to schizophrenic development (4). Consequently, I changed my treatment approach from an interpretative to a fact-finding attitude, with amazingly positive results. For many patients, this was the first time someone had *listened* to what they had to say about how they really felt, instead of telling what they were supposed to feel. In the study of the transference it was recognized that the *giving* of interpretations was to these patients a paralyzing re-experience of something they had been exposed to all their lives, namely: "*Mother* always *knew* how *I* felt; I myself did not know it."

This shift in therapeutic approach resulted in a new formulation of the whole problem, on a different level of psychic functioning—that of perception and conceptualization—instead of the area we usually deal with in psychoanalysis (motivational and emotional expression). It became apparent that these patients suffered from an inability to identify their bodily sensations correctly, in particular hunger and satiation, and from a severe deficit in the sense of identity and self-effectiveness. This defect in body concept (14) and self-awareness seems to be of crucial importance for the development of severe eating disorders, and must be corrected to achieve lasting treatment results.

CASE 1

This young woman, whom I shall call Gail, was selected for detailed report because she expressed with dramatic directness the conceptual confusion in her sense of self-effectiveness and bodily awareness. I regret that I cannot reproduce the full flavor of the grotesque atmosphere of mutual coercion and violence that surrounded the development of the patient. She entered the New York State Psychiatric Institute in October, 1959, at the age of 20, a slim, well groomed and fashionably dressed young woman. Her doctor had made arrangements for hospitalization several weeks earlier, but she had refused to sign the application for voluntary admission. Her weight was 96 pounds, a sacred figure she had rigidly maintained for four or five years. And thereby hangs the tale.

Up to the age of 13 Gail had been plump, for which she blamed her parents. Her weight had reached nearly 140 pounds and had earned her the nickname of Two-Ton Tilly. When she was 11 the parents sought help at a child guidance clinic because she was unmanageable at home. There was no improvement during a year of treatment of both mother and child, and a boarding school with treatment facilities was recommended. During this period Gail was terrified and dared not eat anything because her eating might get out of hand and she would become even fatter. Instead, a seeming miracle happened: she began to grow thinner. She decided also to put aside the much ridiculed name of Tilly, and chose a new one, Gail.

After two years the parents, feeling guilty about the accusation of having got rid of her, and believing her promise of better behavior, gave in to her demands and Gail returned to her home. She was determined to keep her weight at 96 pounds, an insistence that led to her dominating the household with enforced rituals. The parents were obliged to go shopping twice a day be-

cause Gail would not permit any food in the house between meals. Anything left over after a meal had to be thrown away, so she could not succumb to the compulsion of eating it and thereby exceed the magic upper limit of 96 pounds. She entered high school and got good grades by hard study. She also insisted on absolute quiet at home; her parents were not allowed to be there when she was studying.

The parents were unable to cope with the situation and again sought psychiatric help. Gail consented to see the psychiatrist, though on her own terms. Things went along this way, with many outbursts of violence, until she was graduated from high school. She did not want her parents to come to the commencement and refused to get tickets for them. When the mother insisted the parents would go in any event, Gail asked, "How, how?" The mother refused to tell and Gail became increasingly frantic, finally attacking her mother with a pair of scissors and threatening to kill her. The mother ran out of the house to the father's store; they called the police, and Gail was taken to a city hospital. Following this episode she stayed for six months at a private hospital, where medication was required to keep her manageable. After her return home she saw her psychiatrist regularly and became very dependent upon him. Gail also worked as a salesgirl in a number of stores for a year and a half, holding her job in any one place for no more than three or four months. She was considered to be a good salesgirl, but characteristically her methods became so aggressive she got into difficulties. Her favorite item was jewelry. If a customer stopped to look at a piece of jewelry Gail seemed to consider it a challenge, and usually succeeded in persuading the customer to buy.

Her behavior at home remained unchanged, in particular her fixed pattern of controlling the parents with the demand for a restricted diet. The father suggested that, since she was earning money, she have an apartment of her own. Gail's response was: "You can't get rid of me that easily." She thereupon gave up her job and stayed at home. Within two months her violent behavior forced the parents to move into a furnished room; they were careful not to let Gail know their new address. Even after Gail had been at the hospital for over a year the mother was afraid to move back to the old apartment: it reminded her of her daughter's violence, with furniture and fixtures marred and half-destroyed. After the upholstery had been ruined the mother had bought slip covers but Gail had cut them to ribbons. (She also had torn down the shades and draperies.) The parents had endured all this because Gail's psychiatrist implored them to be patient. According to the parents, the doctor had complied with Gail's request not to talk about certain topics—for instance, the word *food* was absolutely forbidden. He also urged the parents never to talk about food, and to comply with the patient's demands.

Going to her doctor's office three times a week was Gail's only reason for leaving the apartment. She also had the privilege of telephoning him whenever she felt anxious. She kept entirely to herself except for calling her parents at their store to give them orders and tell them what prepared food to bring her twice a day, in the exact amount to keep her from gaining weight. If Gail gained as much as half a pound she created a violent scene.

The situation gradually deteriorated, and Gail's doctor recommended that she enter a psychiatric hospital as a voluntary patient. This she refused to do until one day when her mother reacted to her violence by shouting and throwing things. Gail was so terrified by this that she agreed to hospitalization. During the first few months in the hospital her weight remained her chief preoccupation. She had brought her own scale with her and weighed herself every day, refusing to eat if she had gained so much as an ounce. A change took place when she

became emotionally involved with a male patient: if this young man spoke to her, she felt it indicated he was falling in love with her. She used all her manipulative skills, with the predictable result that he openly rebuffed her. Gail was indignant and bewildered that the young man did not respond, even though she was then at the "perfect" weight of 96 pounds. After this rejection she gained weight rapidly (to approximately 150 pounds within two or three months). She now dressed in maternity clothes, with the expressed intent of giving the impression of being pregnant. This to her was less shameful and aroused less guilt than appearing fat. At the same time the idea of pregnancy was revolting to her, as was that of intercourse. Gail said that, if it were not painful, she would have her uterus removed, "just to be safe."

After Gail's first physician left the hospital, she greeted her new therapist with a sarcastic outburst: "I hate you already. You can never be Dr. X. I can see with one look that you are inexperienced, impervious, do not know much psychiatry and maybe you are even a V.A. resident."

Accusations of this sort persisted through several months. Her new doctor felt discouraged, and with reason. Despite more than eight years of therapy, Gail's course had been downhill. The physician asked for help in evaluating the situation, and for supervision: there was the question whether Gail was untreatable.

A review of the previous treatment approaches showed that they had been conducted according to the general principle of avoiding errors made in the past by the parents or by other physicians, and of confronting the patient with her manipulative behavior and power struggle. The underlying conceptual delusion of herself and of all human relatedness had *not* been recognized, and had therefore remained unexplored and uncorrected. Actually, Gail had been unusually clear on this point, but the significance of her frequent statements had been missed. In a conference together with her parents she burst out: "I tried to remake my parents all through my life. I wanted to put them in a different role, make them warm and understanding so that they could raise me better. What I'm now *my parents have created. I'm the product of their creation.* It is all their fault." There was a note of despair when she added: "Don't they see how much I suffer? Don't they see how unhappy I am about being a brat, always fighting and arguing?" Being thin was one way of being something in her own right, something not quite her parent's product. They had always urged food on her and had wanted her to be fat.

The mother had been devoted to Gail's cousin, a cuddly, plump baby, born five years before Gail. When Gail was born the mother was disappointed because the baby was "long and stringy" rather than plump. She set out to change the infant Gail into a dream child, and devoted her life to making her underweight child plump and pleasant.

The outcome was a tragedy of mutual blame and guilt, and the interaction in this family embodied a distinctly Frankenstein theme. The daughter, conceived of as her parents' creation, turned against her creators, but, appearances notwithstanding, was not basically aggressive. She wanted only to change her parents so they would undo their errors and recreate her in a better mold. A primary example of this deep-seated sense of her own helplessness is seen in the bizarre dieting. It may be thought of also as a consequence of her inability to recognize hunger and satiation; hence her demand for control over her bodily needs through her parents.

This new orientation was followed by a decided change in therapeutic progress. Gail accepted this approach more readily than had been anticipated. It was possible to evoke in her an awareness of undeveloped resources within herself, and to make her feel that she could rely on her own initiative and behavior. The social worker's approach

to the parents was also re-evaluated, with so dramatic a change that four months later the parents moved back to their old apartment and redecorated it. Though Gail was in need of continued treatment and not ready for discharge, at this time it could be discussed in realistic terms. It was decided that she should not live with her parents but should have vocational training to equip her to be independent and eventually to live away from home and the hospital.

CASE 2

A professional man in his mid-forties had been forced to curtail his work following a heart attack a year before, and had been urgently advised to reduce. (He weighed 250 pounds.) When I first saw him he had made several brief efforts at reducing and at psychotherapy. After a number of meetings, and in a spirit of bravado, he related the following. He felt that the real failure of his life was not that he lacked the will power to follow a diet, but rather that he lacked the courage to be as fat as he was supposed to be—and would be—except that he had learned to be ashamed of it, and was harassed by the knowledge that it was dangerous for him to be so heavy. He had vivid memories of his early childhood, and spoke with love and admiration of his mother, who had cared for him devotedly. Outstanding in his memory was her fear that he was not eating enough. She served lavish food and stood over him to be sure he ate every bite. He could still hear her—"*Ess, ess, ich sterbe weg* (Eat, eat, or I'll die)"—whenever he tried to go on a diet or began to lose weight. None of his sound medical knowledge protected him against the devastating feeling that by not eating he was killing his mother (who had been dead for some years).

CASE 3

In this young woman the lack of independence was expressed in a minor key. Cynthia, now 21, had suffered from anorexia nervosa when she was 14, her weight dropping below 60 pounds. She seemed to recover, and finished high school when she was 18. At college she began to eat compulsively, became depressed, and left before the end of the first year. She came to New York for treatment and to become an actress. Her family seemed to be devoted to her and to be people of culture. The patient, however, described her background in conventional and defensive terms. She felt her parents had over-protected her and not given her enough confidence in herself. The first hint of difficulty came after a telephone conversation with her father that sounded innocuous enough. He had said: "How are you, darling? How do you like your room? Do you have enough money?" To these questions she had answered, monosyllabically, that everything was fine. Then the father asked: "Are you happy, darling?" This enraged her. What right had he to ask whether she was happy! Just because he gave her money, had he the right to expect that *she* had to be happy *for him*, so he could feel satisfied with himself! She elaborated on this, feeling that his expecting her to be happy had poisoned her life. A series of complaints about her mother followed. Cynthia felt she was being forced to get an education for her mother's sake. She had been unhappy in college because she was oppressed by the question, What am I doing here? Do I have to go to college, just because mother didn't go?

Painstaking work was necessary to make her recognize the extent to which she was robbed of all sense of achievement by her concept that her functioning was in the service of somebody else's needs and not her own. Her desire to become an actress or model expressed her frantic search for a substitute identity in the hope of acclaim and recognition. Gradually she came to recognize that her whole life had been determined by the fear of not being in control of her own feelings and impulses. She had failed to develop an awareness that she had within herself the resources for estab-

lishing such controls. The episodes of compulsive eating terrified her because they were concrete evidence of her failure: there she was, stuffing herself against her will and without hunger or pleasure—only the terror of feeling empty and lost. This happened each time she was disappointed in herself or in someone she thought would deserve her trust and fulfill her needs and wishes.

DISCUSSION

The foregoing clinical material has illustrated two points—the inability to recognize hunger and other bodily sensations, and the lacking of awareness of living one's own life—as of fundamental significance in the development of eating disturbances and associated personality problems. Because of this perceptual and conceptual confusion, an individual may be poorly equipped to orient himself about his body and his social-emotional environment. Deficient in essential tools at each developmental step, he is continuously bewildered and anxious, incapable of effective self-assertion in an incomprehensible world. The gamut of disturbed emotional reactions and defense mechanisms, which are recognized in the psychodynamic approach to these problems, may be considered as resulting from efforts to deal with life and its problems with deficient concepts.

To understand personality development in this orientation, the problem had to be approached on a lower level of complexity. Psychoanalysts have a habit of using adult emotional terms in describing a child's development, and sometimes forget that it is impossible to know how an infant or a young child feels, or from what motives he acts. The same holds true for the use of language in theoretical analytic formulations: words, often colorful analogies designed to cope with complex situations (10), are mistaken for the cause of the conditions they describe.

Understanding the origin of disturbed bodily functions and their relation to mental disorders has been discussed before. Kubie (15) examined in detail the interaction between the biogenetic ingredients in the constellation of instinctual deviates and the symbolic processes.

The psychosomatic literature contains many references to overeating because of feelings of frustration, pregnancy fantasies, or a substitution of food for love and security. These and similar pronouncements are not entirely in error, but they fail to explain why certain people perceive, or misperceive, their bodily sensations—specifically, the nutritional function—in such fashion that they put them to use in complex emotional and interpersonal problems.

Detailed information about early and also later eating habits, in both anorexia nervosa and obesity, suggests that the importance of the function of eating needs to be learned, and that the learning process can miscarry. It has been tacitly assumed that the organism "knows" its bodily sensations and can recognize its "drives"; that, due to homeostatic regulation, unsatisfied needs lead to corrective behavior. This is not so. Recognition of bodily needs and the behavior appropriate to their satisfaction depend to a large extent upon learning, from infancy on. The subcortical centers, which regulate visceral and emotional behavior, receive the "information" on how to do this during this early developmental phase. Neurophysiologists have furnished experimental support for this view. Hebb (13) in his book, *Organization of Behavior*, summarizing his own observations and those of others, stated that lack of food and nutritional deprivation may be disruptive of organized behavior. Even in such an animal as the rat, a certain amount of knowledge must be acquired for the animal to behave appropriately. Hebb pointed out that such learned cognitive factors must be considered when "hunger" or "satiation" is studied in the mature animal.

In human development, because of the

gradually maturing brain function, with its capacity for varying levels of abstraction, and because of the interaction of the biologic field with the social-emotional field, the process becomes exceedingly complex and may miscarry. If things go well, a child learns to identify his body needs correctly, and to satisfy them in ways that are biologically appropriate and at the same time proper for his social and cultural setting. If the innate needs of the child and the environmental responses are to some degree disparate, confusion in conceptual awareness may result.

These people are unable to recognize when they are hungry or satiated, nor do they differentiate need for food from other sensations and feelings of discomfort. They need signals from the outside to know when to eat and when to stop; their own inner awareness has not been programed correctly. On systematic inquiry, an amazing number of fat or thin people will answer with an immediate recognition that this is exactly what they have suffered from all their lives. Their cognitive perception of their nutritional state must be considered to be inaccurate. In experimental studies, Stunkard (25) observed the same phenomenon, which he attributed to a denial of hunger sensations in the face of social disapproval. The subjective experience of hunger in patients with various eating disturbances is being studied in our department (8), with the aid of the gastro-electrograph.

Although experimental data are scant, it seems justified to consider this failure of proprioceptive recognition to be a result of inaccurate learning. Nutrition is of course not the only function that is misperceived and misused in this way; other bodily sensations or interpersonal situations may also be identified incorrectly. Since a disturbance in eating is reflected in changed body size, patients suffering from it are usually classified according to this, their most conspicuous symptom. In patients with disorders of other bodily functions, as, for instance, homosexuality, incorrect conceptualization of various bodily states can readily be detected.

Correct or incorrect learning experiences may be codified on various levels of conceptual representation in the brain, depending upon the stage of maturation. This learning process is not restricted to infancy, but continues throughout childhood. During periods of emotional stress the differentiated and alert regulation of higher brain activity may break down, and lower, incorrectly programed brain centers be called upon, with resulting uncontrolled eating or refusal of food.

The early history often fails to give evidence of gross neglect, and such commonly used terms as rejection or lack of proper love fail to contribute to an understanding of the problem. The details are sometimes quite subtle. What is of importance is whether the response was appropriate to the child's needs, or was superimposed according to what the mother *felt* (often mistakenly) were his needs. Clinical observations indicate that two basic forms of behavior, quite independent of specific content, must be recognized: behavior that originates in the individual, and behavior in response to stimuli from the outside. This distinction applies to the biologic as well as the emotional and interpersonal fields, with a wide range of individual variation in every aspect. Whether an individual grows up to be healthy, or more or less mentally sick, depends upon whether there are appropriate responses to the various expressions of behavior that originate in the child. Only when this is forthcoming can he develop autonomy, initiative, and an adequate sense of trust in his own activities and his effectiveness in social situations and in satisfying his bodily needs. Appropriate stimulation is also part of normal development, but experiences in this field seem to have been less deficient.

In patients in whom the eating disorder is severe, signals of nutritional need are not the only ones that have been inappropriately or inconsistently appeased. Invariably there is evidence that other aspects of child-initiated behavior have been disregarded or inappropriately responded to. Although seemingly obvious, it nonetheless appears to be crucial in the development of any child that there be someone to respond to him and to smile back when he reaches out or smiles at the world. Lack of such appropriate response does not mean that a mother has neglected her child; on the contrary, she may have given him every attention, but in an effort to make him do things she feels he should do. If her response is extremely one-sided, so that only she initiates behavior, her child may grow up living entirely by responding to others, either with over-compliance or with rigid negativism. He will never know that thoughts, feelings or actions originating in him can be effective.

I have focused on the factors underlying severe eating disorders, but disturbances of this sort will also be found in the schizophrenic development, where, in the most severe cases, the patient may feel that someone else is doing his thinking; he may have only the vaguest concept of his own bodily existence and a poorly integrated body image. I feel that incorrect perception of bodily sensations is the link between severe eating disorders and schizophrenia (6).

The therapeutic implications of these concepts are many and far-reaching. They may be reduced to a simple formula: that the essential step in treatment is to help such a patient to become aware that there is something—be it thoughts, feelings, judgments, decisions—that originates within him, and that he can learn to use his own initiative and resources effectively. Failure of self-awareness may extend to many aspects of behavior, and the way a patient can be helped to become aware of his own functioning may seem at first to be insignificant. Sometimes it is useful to draw a patient's attention to his verbal behavior. He may report an event, any event, as "something that happened to me," but never say, "I did" or "I said." By becoming aware of this he may, rather than disclaim his participation, come to recognize an element of personal initiative. He may thus become less passive. Specifically, such a patient needs help in becoming aware of sensations indicating bodily needs. Some have never realized that food is eaten to maintain health and strength; they have used it chiefly for non-nutritional ends, in a losing struggle to establish some sense of identity.

It is a milestone in treatment when a patient recognizes some deficit in his perception and conceptualization and reacts to it in a positive way, as a challenge for further clarification and not as a criticism. When he achieves a degree of confidence in his ability to make discriminating observations and judgments, he is ready to approach and analyze the more complex psychodynamic problems usually dealt with in analysis. These patients must first become aware of and then correct certain aspects of this conceptual confusion. Only then can they use interpretations in a constructive way. Otherwise, psychoanalytic treatment will deteriorate into resistance or intellectualizing.

SUMMARY AND CONCLUSIONS

The importance of emotional factors in eating disorders (both obesity and anorexia nervosa) has been recognized in the past. Psychoanalytic concepts helped to clarify the psychodynamics of disturbed eating behavior and the symbolic significance of food and body size, but were not effective in therapeutic application.

Analysis of the mental processes underlying the motivational factors produced

evidence of conceptual and perceptual deficits. The inability to identify correctly hunger and other bodily sensations and emotional states could be recognized in patients with eating disturbances.

This conceptual confusion may be conceived of as the result of incorrect early learning experiences in the absence of appropriate response to signs and signals originating in the child. While this may apply to any aspect of a child's development, it will result in disturbed weight regulation when the failure of appropriate response is pronounced in relation to the feeding situation. Detailed family studies have revealed the developmental background of such learning deficits.

If failure of confirmation of child-initiated behavior is severe, the result may be a person who lives chiefly by responding to stimuli from others; he may respond with passive compliance or rigid negativism, without experiencing sensations, thoughts or feelings as originating within himself. This same lack of a sense of self-effectiveness, a feeling of being influenced from the outside, also occurs in schizophrenic development.

It is submitted that the therapeutic implications of this approach are far-reaching. Traditional insight-giving psychotherapy reinforces the patient's experience of being influenced by others. Effective psychotherapy must evoke an awareness of self-initiated thinking and acting.

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PROVOCATION OF SUICIDAL ATTEMPTS

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A number of recently published papers on suicide and suicidal attempts have emphasized that the intent to commit suicide is frequently and repeatedly communicated (2–7). This observation generated the present study, which was designed to test the following hypotheses derived from clinical data and understandings gained during intensive psychotherapy with individual patients: 1) that a suicidal attempt is the direct outcome of an interpersonal conflict situation with a specific, psychologically important figure (the "partner"); 2) that it follows, in logical sequence, previous interactions culminating in the threat of suicide; and 3) that this threat must be rejected by the partner before the attempt is carried out.

METHOD

Two groups of ten patients each were studied. One group consisted of patients suffering from psychotic disorders; the other, of patients suffering from psychoneurotic disorders, personality disorders or personality trait disturbances. The definitions of the American Psychiatric Association's *Mental Disorders: Diagnostic and Statistical Manual* (1) were adhered to in making these diagnoses. All patients were seen in response to a request for psychiatric consultation in the general hospital to which they had been admitted following their attempts at suicide. Ten cases each, falling under either the psychotic or the non-psychotic category, were selected in numerical sequence as from the date of the study. In addition to repeated interviews with the patients, some other psychologically significant person with whom the patient was deeply involved at the time of the attempt was also interviewed.

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RESULTS

In the non-psychotic group the following general picture emerged from the interviews:

There was an intense interaction between the patient and a partner, such as spouse, parent or intimate friend. Characteristically, this interaction was a conflict which the patient handled in a very demanding, passive-aggressive, clinging manner, and which culminated in repeated threats of suicide expressed either directly or by almost unmistakable implication. Such a threat at first appeared to be a powerful weapon but on repetition became less effective. Finally, the response of the partner to the repeated threat was a direct or indirect expression of counter-aggression, in the form of a challenge: "I dare you!" In some cases this challenge was taken up and acted upon immediately; in others, the suicidal attempt followed a repeated acting-out of aggressive threat and counter-aggressive challenge. The following case histories may serve to illustrate these points:

CASE HISTORY 1

A 38-year-old white female nurse was admitted following an attempt at suicide. There had been several earlier suicidal gestures of a much milder degree, and repeated warnings to her husband that she was about to commit suicide. On the evening preceding admission, after an argument with her husband, the patient got a butcher knife from the kitchen; this her husband took from her. The same evening she ran across the living-room intending to smash her head against the stone fireplace, but was stopped

by her husband. The patient was a rather frightened, worried person who scheduled her life and the life of her family in an obsessive-compulsive fashion. She had known her husband while she was studying to become a nurse, but had broken off their romance after she was graduated and left their home town to accept employment elsewhere. Somewhat later he had to undergo surgery and was discovered to be a hemophiliac. His condition became critical, and she returned at the request of his family and nursed him back to health. Subsequently they became engaged and later married. The husband was a rather opinionated man, inclined to be dictatorial and irresponsible, but the patient considered him to be a rather weak person and in need of her support. Following the birth of their second child, and as the children grew older, she became progressively dissatisfied with her life as a housewife, felt she was not really fulfilling herself, and wanted to take up her studies again, either as a nurse or in a new career as a teacher. Her husband opposed these plans, and insisted that she remain at home and be there when he came back from work to look after him. The situation culminated in repeated quarrels and finally in the patient's threatening suicide. (She later explained that these threats were really meant to make her husband understand that she desperately needed to change her mode of life.) At first the husband was frightened and promised to accede to her wishes, but actually he continued to oppose her. He objected to her accepting a part-time job as a nurse which would have entailed the family's moving to a different location. He again demonstrated to her his own need for her attention by staying away from his job of traveling salesman on the pretext that he had to help out a friend in their city. The wife again threatened suicide, and finally acted, first by taking a mild overdose of sleeping tablets, then by attempting to stab herself (in the manner described above), which permitted her husband to intervene at the last moment and save her.

The immediate results of the attempted suicide and the subsequent hospitalization were the return of the husband to his job and the intervention of the physician to convince the husband that he must accept his wife's need for at least part-time work.

The following case history throws some light on the basically sadistic, hostile and rejecting attitudes of the partner, never recognized by the patient:

CASE HISTORY 2

A 30-year-old white housewife was admitted to the general hospital following an attempted suicide (jumping out of a rolling car on Christmas Eve). She sustained only minor injuries: a few abrasions and contusions. About her suicidal attempt, the patient stated that she did not really want to die, only to make her husband understand how desperate she felt. Some eight months prior to the present admission she had been admitted to the psychiatric ward of a general hospital with the diagnosis of a postpuerperal depression, had been treated with tranquilizers and discharged in an improved condition.

The interaction between the patient and her husband was described as follows: The patient had felt inadequate in the performance of her duties as wife and mother, especially since the birth of her third child, who was quite demanding. When the husband returned from work in the evening he would find his wife full of complaints. At first he was sympathetic, but as time went by he became annoyed with her, and said she would have to pull herself together. Following her hospitalization for the postpuerperal depression he was more sympathetic, and hired someone to help her. But the complaints continued, he again became annoyed, and the patient responded with increased clinging and demands for love and attention, and finally with threats that she would harm herself. She demonstrated her intention to commit suicide by running

to the kitchen and getting the aspirin bottle. At first the husband intervened, but after a time he got "fed up" and watched "to see what she would do." She put a handful of tablets into her mouth but, when he could not intervene, spat them into her hand and returned them to the bottle.

The patient had also threatened before to jump out of the moving car, and at first the husband had felt frightened and had restrained her. The night of admission she threatened again, and he decided that "there was plenty of snow on the road and high banks of snow on both sides," and so disregarded her threat and drove on. Thereupon the patient opened the door of the car and jumped out.

CASE HISTORY 3

A 44-year-old white housewife was admitted to the general hospital after a second attempt at suicide, the first having occurred the summer before, when she had slashed both her wrists and her neck in the following setting:

Always somewhat nervous, high-strung, active and "on the go," the patient had felt exhausted and lonely after a big wedding given for her eldest daughter. She turned toward her husband, demanding more and more of his attention. After a time, he felt that her demands were becoming unreasonable and were interfering with his work and his life in general. He went to his minister for advice and after a long talk decided he should not devote his entire life to his wife, since he also had obligations to himself and his children. When he told his wife that this was what he was going to do she protested and pleaded with him not to abandon her. He left, saying that he had to go downtown with his son on business, and on his return found that she had attempted suicide. He had her hospitalized immediately.

The second attempt was made several weeks after the patient's return from the hospital. Her doctor had put her on tranquilizing and sleeping tablets, which he had handed to the husband with instructions as to when and how to give them to her. After a few days the husband felt he could not stand the patient's constantly begging for more tablets, and refused to continue to keep the pills. He handed them to his wife, who promptly took an overdose of the tranquilizer.

In the psychotic group, this pattern of interaction between patient and a partner, leading to the suicidal threat and finally to the challenge, was less clearly and regularly demonstrated. The threat of suicide was not expressed clearly, and was sometimes couched in such autistic symbolism that it could not be understood by the partner. Also, the partner in his own right was perhaps less involved than were the partners of the non-psychotic group, and his rejection of the patient was perhaps less conscious and less intense.

CASE HISTORY 4

A 28-year-old white unmarried female attempted suicide by throwing herself in front of an oncoming car. The driver, however, was coming slowly around a corner and had plenty of time to stop, and she escaped serious injury.

The patient, who was diagnosed as suffering from a schizophrenic reaction, chronic undifferentiated type, had been involved in a life-long struggle with her father. Her mother had died when she was six, and on the death of a maternal grandmother, shortly after her mother's death, she was left a substantial sum of money. She felt that her father took advantage of his guardianship by using her money, and also by paying for her tuition and studies out of her money instead of supporting her himself. She had always had a rather inadequate self-concept, and felt extremely insecure. Despite this, she went away to college, and later pursued an independent career away from home. Her father remarried, and she felt he took little interest in her welfare. On her last visit things again did not go well, and she left with the feeling that her parents were less interested in her

than ever. She was particularly angry about their planning a trip to Europe, which seemed to her the final desertion. She had expressed her anger and also, she felt, her desperation about her loneliness. The father was apparently unaware of all this, although later he readily admitted that he had taken no active interest in his daughter since she left home. The patient's final threat was to change her will so that, instead of leaving all her property to her parents, she cut them out entirely. Autistically, the patient felt that awareness of this change of mind with regard to the will must somehow occur to her father. When he did not write or react in any way she felt it was "time to teach the parents a lesson," and attempted to throw herself in front of an oncoming car.

DISCUSSION

Using unselected subjects and differentiating only between psychotic and non-psychotic patients permitted the observation and investigation of a wide range of psychopathology. This range was evidenced also in the great differences of feelings expressed by the patients with regard to their suicidal attempts. At one end of the scale was the patient who clearly made a suicidal gesture as a form of communication, with the goal—on a fairly conscious level—of achieving a "desired effect" (6). The less consciously aware of this goal the person was, the more obscure and autistic the form in which the intent to commit suicide was expressed, and the more severe the suicidal attempt. At the other end of the scale was the patient in whom conscious awareness of a goal played only a minor role, the patient disturbed to the point where primary thought processes predominated. On recovery from an acute psychotic reaction he could not account for his suicidal attempt, had only a very hazy memory of events leading up to it, and expressed feelings of bewilderment over his actions, although he could remember feeling rejected and abandoned.

Results of this investigation confirm the major hypotheses on a clinical level. In the non-psychotic group, the suicidal attempt was clearly a way of communicating. It was an acted-out, non-verbal communication, but it followed earlier verbal communications in logical sequence. Of equal importance, and precipitating the attempt, was the response received from the partner in the relationship, the "feed-back" from the person to whom the communication was directed. The suicide threat had initially brought forth in this partner strong feelings of guilt and compassion, but in the long run, and for reasons which would have to be sought in the partner's own personality make-up, these feelings gave way to the expression of counter-aggression: the direct or indirect challenge to the patient to go ahead. It appeared to be this feed-back which led, either immediately or following a repeated acting-out of threat (aggression) and counter-aggression ("I dare you"), to the attempted suicide. The issue of life and death, of existence versus non-existence, seemed to be entirely lost to view during the course of these interactions.

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A STUDY OF OCULAR MOVEMENTS IN HYPNOTICALLY INDUCED DREAMS[1]

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Interest in the use of the electroencephalographic and eye-movement potentials in the study of dreaming stems from the reports of Aserinsky and Kleitman (1, 2), who observed periods of rapid conjugate eye movement during sleep. Subjects awakened in the presence of these eye movements showed a high incidence of dream recall (74 per cent), and a low incidence if awakened during periods of no eye movement (9 per cent). Dement and others (4–6, 10) have confirmed these findings in normal and in schizophrenic subjects. These investigators found a correlation in the EEGs of very light sleep, eye movements and dreaming.

Dement stated that, when eye movements were directly observed in a sleeping person, "the subjective impression was that the eye movements resembled those of an individual scanning his environment or watching some event associated with a great deal of movement and activities" (4, p. 264). To test the hypothesis that "the movements represented the visual imagery of the dream, that is, that they corresponded to where and at what the dreamer was looking" (5, pp. 343–344), Dement and Kleitman examined the dream content and patterns of eye movement. They found that the predominant directions of the eye movements (vertical, horizontal, or mixed) corresponded to the major directions of activity in the dream, and that movement was minimal or absent when the dreamer was staring at an object or at some distant scene. In a later study Dement and Wolpert (7) correctly "predicted," on the basis of amount of eye movements alone, the participation of the dreamer in the dream activity (categorized as "passive" or "active") in 76 per cent of the records examined.

Such data support the notion of an intrinsic relation between eye movements and dreaming, although Dement states that the occurrence of dreams without eye movement is not ruled out.

The question arises whether these relationships found with dreams of normal sleep hold for hypnotically-induced dreams which occur in a trance state or, through post-hypnotic suggestion, in normal sleep. The correlations noted with the dreams of normal sleep may hold generally for hypnotic dreams. If so, hypnosis, which provides a method for control of both dream content and eye movement, can be used to delineate more clearly the relation between dream and ocular activity in dreams of normal sleep. With hypnosis, not only can the activity within the dream be controlled, but the particular kind of visual orientation of the dreamer to his dream scene can also be specified, to see whether the subsequent eye movements are those oculomotor adjustments that would be expected if the visual experience took place in a waking rather than a sleeping state. The intrinsic relation-

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ship of eye movement to any dream can be probed in hypnotic dreams by testing the degree to which inhibition of ocular movement influences the occurrence or content of the dream. Finally, a study of the similarities and differences between "normal" and hypnotic dreams, with simultaneous recording of EEG and ocular movements, allows an approach to the differentiation of the psychologic organizations in the two kinds of dreaming.

We therefore investigated the relations between EEG patterns, eye movements, and type of activity occurring in the hypnotic dream.

METHOD

One subject was studied intensively in 12 experimental sessions. *S* was a 25-year-old unmarried white girl, an artist, whose extensive experience as a subject in experiments using hypnosis facilitated both the investigative procedures and our interpretation of her behavior.

To record eye movements and the EEG, seven small silver disk electrodes were applied to *S*. The EEG and eye movement potentials were amplified and recorded by a four-channel Medcraft Electroencephalograph. Three of the four channels were utilized in this experiment. The first two channels, recorded in a monopolar fashion from the outer canthus of each eye, referred to the ear lobes of the same side. The third channel gave a bipolar record from the parietal and occipital electrodes. A constant paper speed of three centimeters per second was used, with sample records taken at intervals at a speed of six centimeters per second. The experiment was conducted in a sound-proof room provided with a couch on which *S* reclined, and a one-way viewing mirror behind which recording apparatus was placed. The hypnotist at times remained with *S* in her room, and at other times observed her from the recording room. In all experiments, one or two observers were present in the recording room. *S* participated in the experiment with full knowledge that *E* could see and hear her, and record her behavior.

EEG and eye-movement-potential recordings were made when the eyes were open and closed in the normal waking and hypnotic trance states, and in both states *S* was directed to move her eyes "right and left," "up and down" and "looking straight ahead." The same recordings were then made during the hypnotic induction, the trance state, and during induced dreams of both an active and a passive nature.

During each of the 12 sessions *S* was hypnotized and told to have dreams (either while in the trance state or post-hypnotically); each dream carried specific instructions as to activity. These instructions were varied and were introduced flexibly; for example, some instructions controlled amount of activity within the dream (by suggesting general content—a peaceful scene, a sports event); some controlled the dreamer's activity (playing a game, watching a game, watching a calm scene); some influenced eye movement directly (by suggesting that *S* keep her eyes fixed on a point within the dream scene, or by suggesting that a mask was in front of her eyes). On occasion *S* was instructed while in the trance to signify the beginning and the ending of the induced dream by a motor act (crossing the legs) which could be easily observed through the one-way mirror. This could be correlated with a spoken instruction, the observations of the investigator in the room, and the simultaneous EEG recording.

RESULTS

STATES OF WAKEFULNESS, INDUCTION, TRANCE AND CHANGE OF STATE

The waking state was generally characterized by random eye movements (Figure 1), although on command *S* was able,

FIG. 1. Record of normal random eye movements of *S* awake, with eyes open. Top tracing, right eye; middle tracing, left eye; lower tracing EEG, parietal and occipital leads.

with eyes open or closed, to hold her eyes steady and look straight ahead, and thereby produce a record of minimal movement.

The subject was given the following instructions: "All right now, sit back and relax. Close your eyes. Now you find you are becoming sleepier and sleepier and you are going into a deep trance. Your eyelids are heavy and closed; you are becoming sleepier and sleepier and as I count from one to 20 you will go deeper and deeper into a trance." On almost every occasion *S* appeared to be in a trance immediately after the first sentence of the instructions. At that point the eye-movement records changed from the activity of the waking state to almost no movement whatever (Figure 2). Occasionally a slightly slower change in the eye movements was observed during the induction of the trance. Throughout the 12 hypnotic sessions (which totaled about 48 hours) the trance state itself showed almost no eye movement. Periodic eye movements, similar to those seen during nocturnal dreams, were observed only at times when dreams had been hypnotically induced, and at no other time during the trance. (Figure 3 is an example of the eye movements occurring in a hypnotically-induced dream.) The lack of eye movements during the trance raises problems concerning the nature of orienting during such states. The dependence of *S* on auditory cues and on the actions of the hypnotist is highlighted by this absence of ocular orienting.

Figure 4 is a typical record of *S*'s awaking from a trance. The hypnotist would tell the subject, "In a moment I am going to bring you out of the trance; I am going to count from three to one, and when I get to one you will be wide awake." Usually

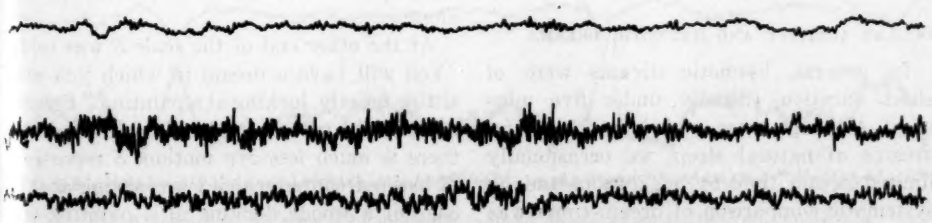

FIG. 2. Record of *S* in hypnotic trance (eyes closed).

FIG. 3. Record of *S* having a hypnotically induced dream.

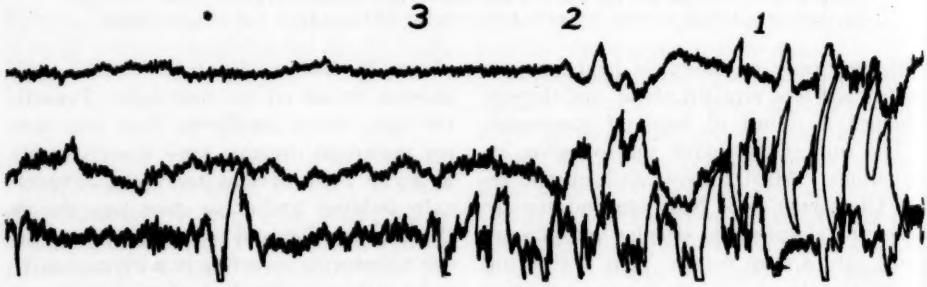

FIG. 4. Record of *S* awakening from the trance state. Numbers correspond to the hypnotist's counting, after instructions that *S* would be awake by the count of "one."

when he said "two" there would be a sharp change in the record.

The characteristic pattern for a change of mental state, then, was an abrupt change in the eye-movement pattern, when *S* was either going into or coming out of the trance. The lack of EEG changes is a usual finding in this and other laboratories (3). It is possible that the period of induction in some other *S*s would be less abrupt, and that studies of eye movement and spontaneous imagery during induction (11) could be carried out.

OCULAR ACTIVITY AND HYPNOTIC DREAMS

In general, hypnotic dreams were of short duration (usually under five minutes). Although these are briefer than many dreams of natural sleep, we occasionally limited dream time by suggestion, and no systematic comparison of dream-times was undertaken. The instructions regarding the dream can be divided into three general categories:

Suggestion of activity or passivity in the dream scene: S was instructed to have dreams in which activity within the scene was high or low. For example, it was suggested to *S* that she have a dream in which she was watching a very active tennis match. Figure 5 shows the tracing of the active eye movement of this dream. *S* reported after awakening, "I had a dream in which I was watching a championship tennis playoff on TV. It was very exciting and close."

At the other end of the scale *S* was told, "You will have a dream in which you are sitting quietly, looking at a painting." Figure 6 shows the tracing of this dream, in which there is much less eye motion. *S* reported, "I had a dream in which I was sitting at the edge of a woods, looking at a painting on my easel. It was very calm." *E* asked,

"Was there any motion in the dream?," and the patient said, "Only a few birds flying around."

Attempt to control eye movement by direct suggestion: It was quite easy experimentally to induce a dream with little activity, but it was difficult to eliminate *all* eye movements. In one attempt *S* was told, "You are inside a multicolored sphere looking up. There will be no motion at all in this dream. There will be no need for your eyes to move." Here *S* reported, "I had a dream I was in a large steel sphere. I knew there was no need to look around, but still I started to explore this inside with my eyes."

In another attempt *S* was told, "You will have a very active dream. It will be pleasant, it will remain pleasant, only as long as you keep your eyes on one spot in front of you" (Figure 7). There is no periodic eye movement in this dream. *S* reported, "I tried to have an active dream, but I couldn't. All I dreamed about was the spot and the bands of color in it. I think my peripheral vision caught flickers of color and shadows, but I couldn't find out what they were."

Control of the dreamer's relation to the dream event: Not only can the dream scene vary in the amount of activity it contains, but also the point of view of the dreamer toward this activity can vary. Two such variations were introduced.

In one, *S* was told a mask covered her face. For example, she was first instructed to dream about a fast Ping-pong match. (Sports matches were selected for the

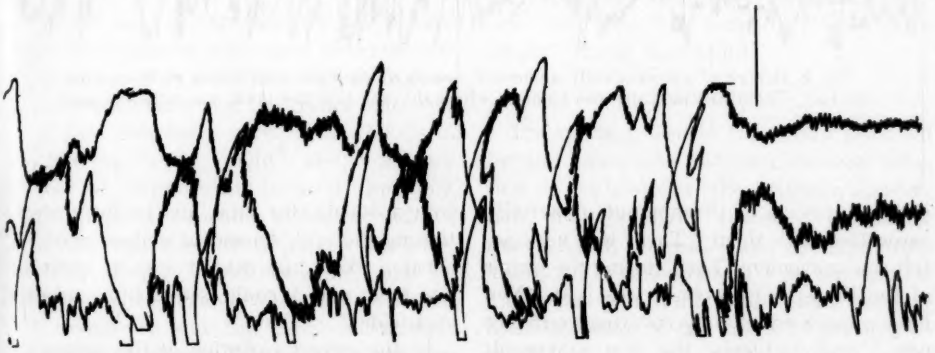

FIG. 5. Record of *S* during a hypnotically induced dream. By hypnotic instruction, the content of the dream was to involve a great deal of activity. The vertical line indicates the end of the dream.

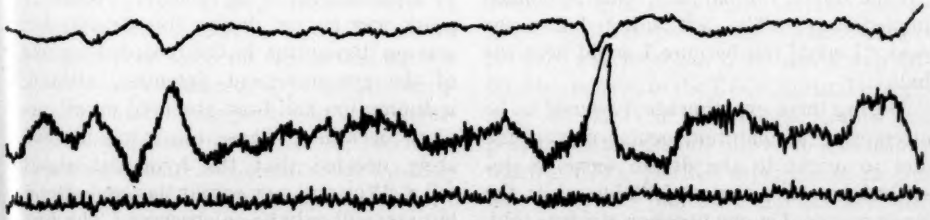

FIG. 6. Record of *S* during a hypnotically induced dream which by hypnotic instruction was to involve minimal activity in the dream.

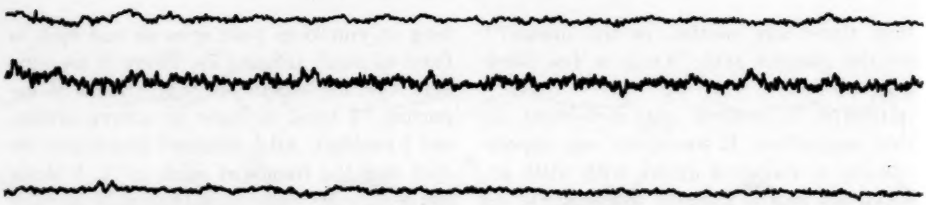

FIG. 7. Record of hypnotically induced dream in which *S* was told she was not to move her eyes.

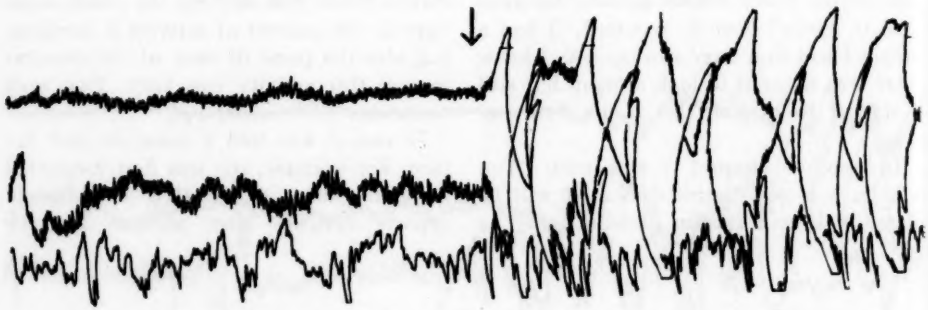

FIG. 8. Record of a hypnotically induced dream in which *S* was told that a mask covered her face. The arrow indicates the point at which she was told the mask was removed and she could see.

dreams because of the amount of activity associated with them.) There was very active eye movement. Then, during the course of the Ping-pong match, *S* was told, "You have a mask completely covering your face now," and suddenly the eye movement stopped. When the mask was taken off *S* was told, "You can now see." As shown in Figure 8, the eye movement abruptly returned. While the mask was on *S* was asked, "What side is the ball on?" She responded immediately, "The left side." Later she said, "I could tell because I could hear the ball."

During these experiments *S* seemed to be attempting to maintain ocular movements and to orient to the dream scene. *S* described herself as uncomfortable while the mask was on. On one occasion she was told, "You now have a mask in front of your face." A few periodic eye movements continued during this time, and on later questioning she said, somewhat embarrassed, "I cheated. You just said it was in front of my face, and I could see a little around each side."

In the second variation of the dreamer's point of view, *S* was instructed either to witness or to participate in the activity. For example, *S* was told, "You will have an active dream of a volleyball match. You may be a participant or an observer." When the mask was put on during this dream there was no diminution in the recorded tracings of the eye-movement potential, although a diminution had been observed on all previous occasions. Those observing immediately decided that the hypnotist should tell *S*, "You will now repeat the same dream but you will only be an observer." The hypnotist was signaled, returned to *S* with the new suggestion, and *S*'s second hypnoti

dream commenced. This time when the hypnotist suggested that the mask would cover the face, the result was a complete cessation of all eye movements. Later, upon release from amnesia, *S* was asked, "Do you remember any dreams?," and she responded, "Yes, I had two dreams. In the first one I was in a good volleyball game. It was very exciting. When you put the mask on I was afraid the ball was going to come down on my head and so I looked around whenever I heard it bounce. It was quite a hassle. In the second dream, I was just watching the game." In these examples the procedures allowed immediate recording of events and immediate manipulation of variables during a single hypnotic session.

SLEEP AND POST-HYPNOTICALLY SUGGESTED DREAMS

In this kind of experimental use of hypnosis we observed differences between natural sleep and hypnotically suggested sleep, and between these and the trance state. We also saw differences between the EEGs of dreams of "every night" sleep and the EEGs of hypnotically-induced dreaming. Apparently, when trance states are induced with the suggestion of sleeping, *S* hears this as "like sleeping," and stays alert and responsive to the events which unfold during the hypnosis, and the EEG accordingly shows wakefulness. Trance states can neither psychologically nor physiologically be directly equated with naturally occurring sleep.

During one trance, it was suggested to *S* that she go into a deep *natural* sleep 15 minutes after the trance. The EEG record indicated deep sleep for two hours, but the expected appearance of a period of conjugate eye movements and associated lightened sleep did not occur, although such periods do characterize normal sleep. Possibly "suggested sleep" differs from normal sleep, although further data will be required to establish this difference.

The occurrence of a post-hypnotic dream was observed under these circumstances. The specific dream was to occur one-half hour following deep sleep; it did not. Rather, after *S* had been deeply asleep for two hours, the hypnotist left the recording room and walked through a corridor to join her in the experimetal room. With the click of the door of the recording room, which was always heard prior to the hypnotist's entry into the experimental room, the EEG showed arousal and a waking rhythm, and within ten seconds rapid conjugate eye movement appeared, a picture characteristic of the hypnotic dream of the trance state. When within a few seconds the door to *S*'s room was opened, the eye record shifted to a normal recording of the awakening state. *S* reported that she had just *begun* to dream, and was awakened by the hypnotist's entry. The recordings indicate that she was actually "electrically awake" during the dream.

DISCUSSION

The method used in this study provided for the immediate and simultaneous notation of *S*'s behavior, the hypnotic instruction and the EEG tracings. Immediate knowledge of the results and the hypnotist's ability to resume contact with the subject make the procedure flexible and immediately sensitive to the needs of the experiment.

In the relationship between "sleeping" and "hypnotic" dreams, it is clear that dreaming during a hypnotic trance, dreaming during sleep as a post-hypnotic task, and dreaming during natural sleep share certain characteristics with respect to the phenomenal experience and to ocular movements, and differ with respect to the EEG. Natural dreams have the EEG of light sleep; trance and post-hypnotic dreams have an essentially waking EEG. Earlier experiments in this laboratory (8) indicated that the performance of all post-hypnotically suggested acts (whether motor acts or dreams) which were to occur during a natural nocturnal sleep

was accompanied by a return of the *S* to his trance state, as defined by his EEG pattern.

The difference in the mental states in which the two types of dreams occur leads one to suspect that the organization or content of the dreams might differ, and evidence for this has been documented (9, 12). Both states, however, seem to yield the same *phenomenal* experience of *dreaming*: though *S*s can differentiate the *state* in which the dreaming occurs, our *S*s do not spontaneously differentiate the *dreams* which occur in a trance or sleep state. Furthermore, hypnotically-induced dreams often seem to involve traditional elements of dream work, as seen most clearly in the finding of day residue in recovered post-hypnotically suggested dreams. It is assumed here, then, that these differences in the EEG reflect differences in special states of consciousness and in the conditions which induce such states, rather than differences in the essential nature of dreaming. To elucidate differences between dreams dreamed in various states of consciousness, the psychologic structure of the dreams and of the states, and interactions between them, should be studied (*vide infra*).

The results of this experiment agree with those of Dement: eye movements similar to those of the waking state occurred during hypnotic dreams, and the frequency and amplitude of these movements were related to the amount of activity in the dream. It was difficult for this *S* to eliminate all eye movement when presented with a visual scene in her dream, even when fixation was directly suggested. *S* seemed to have a strong need to orient herself within the perceptual field; she used sound as a cue to place and identify activities when looking was prohibited. The feeling of uneasiness she reported when she was instructed to have motionless dreams with no eye movement may stem from the implicit prohibition against orienting—even static scenes seemed naturally to compel some scanning and orienting. She seemed to wish to scan the field, and this she could not do without moving her eyes: that is, she could not simply shift the dream scene in front of her eyes to explore the visual field of the dream. *S* also asserted that, when told to have an active dream with no eye movement, she tried to but could not without letting her eyes move.

This relationship between visual orientation to the field of view and eye movements was apparent in several variations of the experiment. When a mask was placed over *S*'s eyes (by suggestion), no eye movement was seen, though the activity of the dream (for example, a Ping-pong game) continued. When she believed she could glance around the sides of the mask, some eye movement occurred. The clearest example of the movement's being an orienting response of the eyes to the total dream *situation* (and not just to the visual field) was that of the dreamer's being in the midst of a volleyball game, with a mask over her face. Trying to avoid the ball, she showed strong eye movement, her eyes probably turning to the perceived source of the noise. It is interesting that this response of the eye (presumably to sound) occurred, inasmuch as actual body movement is said to be absent or minimal during an episode in a dream of normal sleep (7).

These findings substantiate Dement's idea that eye movements are "a specific expression of the visual experience of dreaming" (5, p. 339). We might say that whatever *eye* movements would be expected in the waking state with such an experience probably appear in dreams. As such, the *active participation* of the dreamer is not required for active eye movement—as in the dream of viewing the tennis match on TV, or in Dement's transcript of the dreamer's watching people throwing tomatoes at one another (5). Prediction, then, of the dreamer's activity and participation in the dream solely on the basis of eye movement would be subject to error in those dreams

in which a dreamer "passively" watches a situation requiring a great deal of scanning or following by the eyes. This might account for some of the error by Dement and Wolpert (7), who misjudged 34 per cent of the dreams which had a great deal of eye movement as involving participation by the dreamers. (Their transcripts showed them to be "passive.") They misjudged only 11 per cent of the dreams with "passive" eye movement records. If one broadens the definition of activity, it may be that eye movements do correlate more precisely with an "active attitude" of the subject, in the sense either of his own participation or of his ability to create, "project" or tolerate a great deal of movement in his dream scene.

The several lines of evidence presented here point to the intrinsic relationship between eye movements and dreams, and so substantiate the correlation between the two that was found by various investigators. The difficulty in producing dreams with no eye movement casts doubt on the occurrence of any but the most unusual dreams when no eye movement is observed. Investigation of the visual vividness of a dream and of psychologic factors determining a dimension of intensity in dreaming might further delineate the issue.

The use of hypnosis in studies of dreaming can provide an opportunity to investigate a number of problems in further detail. Dement has proposed that a certain amount of dreaming may be essential to health. He observed that there is a regular pattern and duration of dreaming characteristic for each individual, and that there is a marked increase in dreaming following dream deprivation. It should be possible to determine whether dreaming during a state of sleep could be suppressed by direct hypnotic suggestion, or whether normal patterns of nocturnal dreaming are altered by "satiating" a subject with trance-state dreaming. The amount of eye movement which appears during dreams of the sleeping and trance states in *S* should also be compared. Such investigations would further define the relationships among the various kinds of dreaming.

SUMMARY

The relationships between EEG patterns, eye movements and type of activity in the dream were investigated in dreams occurring in a hypnotic trance and in normal sleep through post-hypnotic suggestion.

The EEGs of both trance and post-hypnotically suggested dreams represented the essential pattern of the waking state.

The positive relation between amount of eye movement and activity in the hypnotic dream agrees with the results of Dement and others. In addition, by suggesting several variations in the dreamer's relation to her dream, the experimenters obtained further evidence that eye movements represent necessary oculomotor adjustments to the situation presented by the dream.

The use of hypnosis in the study of dreaming offers the possibility of controlling the amount of eye movement and the content of the dream, and is a particularly flexible method, sensitive to the immediate needs of an experiment.

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AN EXPERIMENTAL ANALYSIS OF THE HALLUCINATORY-DELUSIONAL TYPE OF REACTION

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INTRODUCTION

Kretschmer (5) pointed out the existence of a personality dimension—"cyclothymia-schizothymia"—the extremes of which are schizophrenia and manic-depressive psychosis. On the basis of this hypothesized dimension, which assumes a continuity between the "normal and psychotic" mental states, Eysenck (2) experimentally demonstrated the existence of a "normal-psychotic" continuum or "factor of psychoticism." This factor is completely independent of the "neuroticism" dimension. The tests used by Eysenck were for the most part selected empirically, with a view to discriminating, at a purely operational level, between the groups studied (100 normal persons, 50 schizophrenic patients and 50 manic-depressive patients). But a few dissociation tests, chosen from among those previously used by Kretschmer, were included in the battery to prove the latter's hypothesis that schizophrenics have a greater capacity for dissociation (a hypothesis at the root of his theory of the "cyclothymia-schizothymia" dimension). The existence of this dimension was not demonstrated in Eysenck's investigation.

The empirically determined structural differences between psychotic patients and clinically normal people which underlie the psychoticism factor seem to us remote from the criteria commonly used in the clinic to distinguish between the two groups. For instance, Eysenck demonstrates that psychotics have a lower score in such tests as fluency, continuous addition, mirror drawing, reversal of perspectives, concentration and memory, as well as a tendency to make more ample expressive movements and to overestimate distances. This does not help us to form any satisfactory hypothesis as to the more specific differences between psychotics and non-psychotics, since it is highly probable, for instance, that the performance of patients with a psycho-organic syndrome would also be below normal in the same tests.

Subsequently, Eysenck *et al.* (3) applied an extensive battery of simple and complex perceptual tests that were useful for purposes of "military, industrial and educational selection, and more particularly in 'screening' potential neurotics and psychotics." The simple tests were again applied empirically to discriminate between non-psychotics and psychotics, while the use of the complex ones was based on the hypothesis (taken from German literature on the subject) that abnormal groups, whether neurotic or psychotic, have a more "analytical" or "disintegrated" attitude. We find this hypothesis too general to permit further progress to be made in the structural analysis of psychosis. The study in question is particularly important for our work because it uses six of the tests applied by us (Street, Dotted Outlines, Gottschaldt's Figures, Cube Design and the Müller-Lyer and Sanders tests). The authors conclude with an appeal for an intrinsic improvement in the quality of the tests employed, but add: "More urgent however, than improving along psychometric lines the practical diagnostic possibilities of these tests is the development of theoretical models of mental abnormality to explain the observed phenomena."

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We decided the first step in an experimental study of the structural differences between normal persons and psychotics should be both a clinical and an experimental study of the mental functions affected in the simplest of the psychoses—paranoia—in which only structural changes in judgment occur, as manifested by delusions. The next steps would be an analysis of the hallucinatory-delusional type of psychosis, and then of the schizophrenic type, in which delusional, hallucinatory and disgregatory disturbances are combined with definite thought aberrations. In practice, however, cases of pure paranoia are so rare that we had to begin our experimental analysis by studying the hallucinatory-delusional reaction type.

In its purest form, this reaction is represented clinically by paraphrenia and the alcoholic hallucinosis syndrome, the evolutions of which are chronic and acute, respectively. In these two syndromes, there are hallucinations and delusions without other, deep-rooted personality dysfunctions. Moreover, in a group of factorial studies on descriptive aspects of functional psychoses, one outstanding factor, characterized by the presence of delusions and various types of hallucinations, was constantly being encountered by Moore (7), Thurstone (9), Wittenborn *et al.* (13–15), Degan (1) and Guertin (4).

This paper reports a preliminary experimental analysis of the possible psychologic functions implicit in the hallucinatory-delusional reaction type. An endeavor was made to study structural psychologic differences in a group of patients suffering from a hallucinatory-delusional psychosis in the active phase, and to compare that group with a clinically normal control group. The experimental group consisted of paraphrenic patients.

The following aspects will be explored:

Visual perception on the basis of certain hypothetic differences between normal persons and psychotics suffering from hallucinations in relation to specific configurational and illusory factors of perception.

Greater prevalence of a paranoid attitude in psychotic persons. Granted that these patients have delusional ideas of a paranoid type, an attempt was made to objectify this attitude by means of special tests.

MATERIAL AND METHOD

SUBJECTS

Characteristics of the experimental group: The group consisted of 20 patients with a diagnosis of paraphrenia but no organic cerebral symptoms. Their maximal age was 65, they had normal visual acuity and color perception, their schooling ranged from the first year of primary studies to the fourth year of secondary school, and their social status was that of either the working or middle class. Their average age was 53.2 years and their average I.Q., 89.3. Nearly all had been hospitalized in the Hospital Psiquiátrico or the Clínica Psiquiátrica Universitaria at Santiago, for an average of 13.9 years.

Characteristics of the control group: This group consisted of 20 persons whose histories revealed no psychotic episodes or organic symptomatology. The other characteristics (maximal age, visual acuity, etc.) resembled those of the experimental group. The average age was 51.85 years; the average I.Q., 90.1.

The characteristics of both groups are summarized in Table 1. The groups were equivalent as to number, sex, schooling, vision and social status. The slight disparities in age and I.Q. are considered unimportant.

TESTS APPLIED

Tests of visual perception, taken from *A Factorial Study of Perception*, by Thurstone (10). The tests selected relate to factors A, B and E as described by Thurstone, and represent:

A. Intensity of perceptual configuration.

TABLE 1

Characteristics of Groups

Group	N	Sex	Age		Education		Intelligence (abbreviated Wechsler)		Vision
			M	σ	Minimum	Maximum	M	σ	
Control	20	F	51.85	6.86	I Primary	IV Sec.	90.10	16.94	Normal
Psychotic	20	F	53.25	6.39	I Primary	IV Sec.	89.25	14.42	Normal

B. Intensity of the illusory deformation of geometric figures.

E. Manipulation of two configurations simultaneously or consecutively.

Factor A: Our working hypothesis was based on the assumption that if a psychotic patient was capable of hallucinating, his real perceptions would be less solid and his hallucinatory perceptions could therefore be more easily intermingled. Psychotics would have a lower perceptual configuration capacity, and their scores in these tests would therefore be lower than those of the control group.

Gottschaldt's figures: The subject is shown a simple figure, such as a cross or number, and asked to find it as part of a more complicated figure. *S* is told that each of the simpler figures is reproduced in the more complicated figure in the same position and size as in the example. *S* must be able to remember one or more given figures in order to identify them rapidly in the more complex figure. *Score A:* Number of drawings correctly completed within the allotted time (two, one, three, four and four minutes, respectively, for parts 1 to 5 of the test). *Score B:* Number of drawings correctly executed per minute of the allotted time. *Saturation* in Thurstone's factor A: score A, 0.51; score B, 0.44.

Cube design: *S* is given a model to be copied with cubes. When arranging the cubes, he has to retain a visual image of the model. In this test the two Wechsler demonstration designs were used, and the seven test designs. Thurstone added design #8, which corresponds to design #17 in Koh's tests; the test thus consists of two demonstration and eight test designs. *Score:* Sum of the times for the last five designs. *Saturation* in Thurstone's factor A: 0.50.

Hidden digits: The test consists of 13 pictures projected consecutively on a screen in a dark room. Since these are microfilm negatives, they appear on the screen as white on black, representing a series of letters or numbers traced in dots, which *S* has to pick out from a background of more dots. *S* must see a configuration in a nonstructured design and, to do so, must recall the partial shape perceived as he examined the rest of the picture. *Score A:* Number of items with a response time of three seconds or more, including the items missed. *Score B:* Number of items missed. *Saturation* in Thurstone's factor A: 0.36.

Street: This is another test in which *S* must build up a perception from an ambiguous stimulus. The test consists of 23 pictures projected (black on white) on a screen; they are shown one at a time, and *S* has to indicate what they represent. There are no irrelevant dots, and all of the elements in the picture are used to form the figure. *Score A:* Number of pictures with a response time of three seconds or more, including the items missed. *Score B:* Number of items missed. *Saturation* in Thurstone's factor A: 0.35.

Dotted outlines: This test is composed of 16 pictures projected consecutively (black on white) on a screen. Each comprises a

series of dots which *S* must use to form a letter or number. This test is psychologically similar to the Hidden Digits test except that all of the dots are an essential part of the shape to be detected. *Score A:* Number of items with a response time of two seconds or more, including the items missed. *Score B:* Number of items missed. *Saturation* in Thurstone's factor A: 0.35.

Factor B: Our hypothesis was that psychotic persons would have a greater facility for distorting the images and projecting subjective aspects onto external perceptual reality. (Hallucinations would be an extreme form of this process.) We assumed that their performance in these tests would show a greater degree of illusion than that of normal *S*s. All of the tests in this group are of optical illusion.

Müller-Lyer: The test is administered by showing *S* a battery of 20 cards, each bearing a Müller-Lyer illusion design. *S* is asked to decide whether the right-hand section is longer or shorter than the left-hand in each case. The designs are reproduced in the form of photostat negatives (white on black), mounted on cards of 17 by 26 cm. The score is the number of times the right-hand section is considered to be longer. A low numerical score points to a relatively greater degree of illusion. *Saturation* in Thurstone's factor B: 0.57.

Sanders: The 15 drawings are reproduced as photostat negatives (white on black), mounted on cards of 17 by 26 cm., shown to *S* at random. In each case *S* must decide whether the oblique line on the right-hand side is longer or shorter than the other oblique line. The score is the number of times *S* considers the right-hand line to be longer. A low numerical score points to a relatively greater degree of illusion. *Saturation* in Thurstone's factor B: 0.55.

Poggendorf: This test is applied in the same way as the other optical illusion tests. The 23 drawings are shown to *S* on a special support, and he must decide in each case whether the right-hand line is lower or higher than the left-hand line, when regarded as a continuation of the latter. The score represents the number of cards with respect to which *S* replied "higher." A high score indicates a high degree of illusion. *Saturation* in Thurstone's factor B: 0.51.

Ehrenstein's square: This test contains 22 drawings reproduced as photostat negatives (white on black) and mounted on cards of 17 by 26 cm. *S* is asked to say in the case of each design whether the right-hand line is longer or shorter than the left-hand line. The score represents the number of times the left-hand line is reported to be longer. A low score indicates a high degree of optical illusion. *Saturation* in Thurstone's factor B: 0.31.

Titchener's circles: The test is composed of 12 drawings in the form of photostat negatives (white on black) and mounted on cards of 17 by 26 cm. *S* must decide whether the central circle on his right hand is larger or smaller than the other central circle. The score is the number of times the right-hand circle is seen as smaller. A high score in this test implies a considerable degree of optical illusion. *Saturation* in Thurstone's factor B: 0.24.

Factor E: Psychotic patients, faced with a given gestalt, form a new one which would constitute the hallucinatory image. Since, in the two tests that follow, one gestalt has to be broken down in order to form another, psychotics are expected to give a better performance than are normal persons.

Hidden figures: The test consists of two trial plates and one test plate in which *S* has to find six hidden figures, human or animal, without taking into account the figures that can be perceived at first glance. Two test scores are recorded which differ from those suggested by Thurstone: 1) the time divided by the number of figures found (time limit, ten minutes), and 2) the number of additional figures discovered.

Form-color memory: The test consists of ten pictures projected on a screen in a dark room, one at a time, for 0.04 second. Each

ontains four designs of different form and olor. *S* must remember as much as he can f each design, with specific reference to orm and color. A good performance de- ends upon his ability to notice colors and hapes simultaneously. Score equals Form lus Color. *Saturation* in Thurstone's factor E: 0.38.

Prevalence of a paranoid attitude. Sears's 8) questionnaire and a test of perceptual ontent specially adapted to this purpose were used.

Rorschach faces: Number of faces seen in the first Rorschach test plate during the ten minutes allotted (five minutes of spontaneous replies and five minutes after being instructed to look for faces). Paranoid *S*s are expected to see a larger number of faces than are normal persons. *Score:* Number of faces observed during a ten-minute presentation.

Sears's auto-reference questionnaire: This consists of 45 questions to which *S* is instructed to answer Yes or No. If the question is meaningless for *S*, or if he finds it too difficult to answer by a simple negative or affirmative, he is told to put a question mark instead. Psychotics are expected to give a larger number of auto-reference replies as part of their paranoid attitude. *Score:* Number of auto-reference replies underlined.

RESULTS

Differences between the experimental and the control groups.

Visual perception: Results are summarized in Table 2.

With respect to the hypothesis formulated at the beginning of this report (that our psychotics would have a lower perceptual configuration capacity), it appears from Table 2 that all of the results of the Gottschaldt and Street tests, and score B of the Hidden Figures test, support the hypothesis. The other scores corresponding to Factor A also point to a lower configurational capacity in the psychotic group, although the differences between this and the normal group did not reach the requisite level of significance. In the Street test the psychotic patients showed a qualitative tendency to select isolated parts of the figure rather than to consider the whole configuration, and to give more replies with destructive content, *e.g.*, death, accidents.

Conversely, the differences between the group means in the case of the Factor B tests were not significant. In addition, the scores of three of the tests (Müller-Lyer, Ehrenstein and Titchener) differentiate the two groups in a way counter to our working hypothesis. Only one of the scores for Factor E (that for Hidden Figures B) confirmed the initial postulate that psychotics have a greater capacity for manipulating two perceptual configurations simultaneously or consecutively. Score A for the Hidden Figures test, as well as those for the form-color test, flatly contradicts the hypothesis, but without, in the case of the latter, reaching a significant level.

Prevalence of a paranoid attitude: Table 3 gives the results of the two tests used to measure this attitude.

This table shows that the first test refutes the working hypothesis in that clinically normal persons saw more faces in the first Rorschach test plate than did psychotics. The second test, on the other hand, bore out the assumption that auto-reference is more intense in psychotics than in normal persons.

Correlations between the tests reaching a significant level of group difference and intelligence.

Table 4 shows the values of r obtained for each of the two groups.

In the control group, the r values in Gottschaldt's test, score B, and in the Hidden Figures test, score B, are statistically significant. In the experimental group, the tests in which r reached a significant level are Gottschaldt's figures, score A, and Hid-

TABLE 2

Differences Between Means of the Two Groups in the Perceptual Tests

	Groups				t	p
	Control		Psychotic			
	M	σ	M	σ		
Intensity of perceptual configuration: Factor A						
1) Gottschaldt's figures						
Score A	4.50	4.54	2.05	3.12	4.30	0.001
Score B	0.29	0.19	0.11	0.26	3.21	0.01
2) Cube design	4.50	4.20	3.60	4.41	0.66	N.S.
3) Hidden digits						
Score A	9.05	2.01	9.75	1.44	1.27	N.S.
Score B	4.55	1.20	5.80	1.66	2.72	0.05
4) Street						
Score A	19.20	1.96	21.50	1.62	4.04	0.001
Score B	12.30	3.06	16.40	3.10	4.20	0.001
5) Dotted outlines						
Score A	12.70	2.28	13.60	1.59	1.41	N.S.
Score B	4.80	2.91	5.40	2.70	0.67	N.S.
Intensity of the illusory deformation of geometric figures: Factor B						
6) Müller-Lyer	10.50	1.59	11.25	1.44	1.56	N.S.
7) Sanders	13.15	2.88	12.05	2.59	1.56	N.S.
8) Poggendorf	15.50	3.85	17.75	3.87	1.27	N.S.
9) Ehrenstein's square	12.40	2.45	13.35	1.27	1.53	N.S.
10) Titchener's circles	10.85	2.53	9.55	2.82	1.52	N.S.
Manipulation of two configurations simultaneously or consecutively: Factor E						
11. Hidden figures						
Score A	48.40	50.75	170.89	123.56	4.10	0.001
Score B	1.30	2.66	4.05	3.40	2.86	0.01
12. Form-color memory	40.15	5.61	35.70	10.70	1.77	N.S.

TABLE 3

Differences Between the Means of the Two Groups in the Paranoid Attitude Tests

	Groups				t	p
	Control		Psychotic			
	M	σ	M	σ		
1. Rorschach plate #1	5.85	4.70	1.90	2.38	3.34	0.01
2. Sears's auto-reference questionnaire	12 40	7.66	20.92	12.00	2.68	0.05

TABLE 4

Correlations Between the Tests Reaching a Significant Level of Group Difference and Intelligence

Test	Control group	Experimental group
Gottschaldt's figures: Score A	0.22	0.58*
Gottschaldt's figures: Score B	0.54*	0.21
Hidden digits: Score B	0.27	−0.49*
Street: Score A	−0.33	0.39
Street: Score B	−0.26	0.38
Hidden figures: Score A	−0.12	−0.11
Hidden figures: Score B	−0.44*	−0.07
Auto-reference questionnaire	−0.36	−0.04
Rorschach plate #1	−0.18	0.03

* $p < 0.01$.

den Digits, score B. In the non-significant correlations, the regression is linear.

DISCUSSION

VISUAL PERCEPTION

Factor A: Intensity of the perceptual configuration: Three of the five tests used to measure this factor showed a significant difference between the two groups, thus supporting the hypothesis. The results showed that the experimental group's performance was inferior to that of the control group. As was assumed in the hypothesis, the paraphrenics gave a larger number of incorrect replies, and were slower to identify an incomplete sensory stimulus, than were normal *Ss*.

Contrary to the results obtained by Eysenck *et al.* (3) working with groups of 20 neurotic subjects, 20 psychotics and 106 controls, in our investigation the Cube Design test failed to reveal any significant difference. Nor did the Dotted Outlines test reach a significant level, the result in this case coinciding with that noted by Eysenck. The Street test was the most effective in revealing a significant difference, reaching a level of 0.001 in the two scores made; this coincides with the considerable saturation of the Street test in Thurstone's factor A, and contradicts Eysenck's results (that no significant difference was detectable among the three groups). Gottschaldt's test, which has a saturation of 0.51 in Thurstone's factor A, yielded no significant differences in the investigation made by Eysenck *et al.* In the present study, score A obtained a significant difference at the level of 0.001, and score B at 0.01. The disparities between these findings and those of Eysenck *et al.* may be explained by the fact that they worked with psychotics of all kinds, probably including manic-depressives, who do not usually suffer from sensory-perceptive disturbances. The significance of our findings may derive from the fact that persistent hallucinations were clinically present in every patient.

Factor B: Intensity of the illusory deformation of geometric figures: The hypothetic assumption that psychotic *Ss* would have a greater illusory capacity, which would be at the root of their sensory-perceptive disturbances, was not confirmed. The five tests used to measure this factor failed to detect any significant differences. In this respect, it should be pointed out that Eysenck *et al.* (3) formulated a contrary hypothesis. On the assumption that normal persons would have a tendency to integrate and synthesize, whereas psychotics would prefer to analyze, they expected the former to show a greater illusory capacity, since illusion involves integration in the sense that the part distorted must be integrated with the rest of the figures. They used the Müller-Lyer and Sanders tests, and obtained a significant difference at the level of 0.01 with the former; the Sanders test, on the other hand, revealed no difference between the groups. Their results do not agree with those obtained by Wachter (12) in relation to the higher degree of illusion in psychotics as compared to clinically normal persons in both the Müller-Lyer and the Sanders tests.

Factor E: Manipulation of two configurations simultaneously or consecutively: Of the tests used to measure this factor, the

only one to reach a significant level was the Hidden Figures test. Score A contradicted the hypothesis: contrary to expectations, the control group found more hidden figures than did the psychotic group. This might be because the discovery of figures requires the formation of configurations not immediately apparent in the stimulus. From the results obtained by other tests included in the battery (Hidden Digits, Street, Gottschaldt's Figures), it is clear that psychotics find it difficult to build perceptual figures of good form quality from a few elements. In the course of the Street test, psychotic *Ss* were seen to show a qualitative and fairly marked tendency to disregard the overall configuration, and instead to choose isolated parts of the figure projected on the screen. When there is an exigency in the stimulus presented (in the present case, the obligation that *S* find certain figures in a given design), psychotics have much greater difficulty. When the material leaves greater scope for initiative, psychotics appear to have a greater facility than do normal *Ss* for discovering figures. In general, our psychotic group would seem to have been more productive when the test involved the formation of perceptual images based on nonspecific stimuli and did not call for a specific performance. This is corroborated by the greater number of supplementary figures of poor form quality pointed out in the Hidden Figures test.

GREATER PREVALENCE OF THE PARANOID ATTITUDE

Results obtained from Sears's auto-reference questionnaire fully confirm the clinical findings with respect to the increase of auto-reference in paranoid patients.

The first Rorschach's test plate was chosen for our investigation because of the variety of clear to dark shades that it presents, and because the figures cut out along the margin are most effective in stimulating face replies. Nonetheless, the results contradict all of the affirmations made in clinical studies on paranoid patients who have taken this test, since, contrary to our hypothesis, *Ss* in the control group noted many more faces than did those in the experimental group.

The results bear out the observation that normal persons have a more intense power of perceptual configuration than do psychotics, because they can more easily form visual perceptions when they have a few elements of reference at their disposal with which to build up the whole configuration. The results also corroborate the observation that normal *Ss* detect more hidden figures than do psychotic *Ss*.

CORRELATIONS BETWEEN THE TESTS REACHING A SIGNIFICANT LEVEL OF GROUP DIFFERENCE AND INTELLIGENCE

Most of the correlations in both the control and the experimental group failed to reach a significant level.

In Gottschaldt's test, which calls for the use of spatial ability, a high correlation with measured intelligence was expected. The same situation is confirmed by the high negative correlation with score B of the Hidden Figures test, since additional figures have a poor form quality.

GENERAL COMMENTS

Our results cannot easily be integrated within a comprehensive structural hypothesis on psychosis that would cover both the level of perception and the paranoid attitude. We should like, however, to make some observations in this respect, since experiments recently carried out by the senior author on degrees of psychoticism and the threshold of appearance of psychosis induced by LSD-25 have led to the formulation of a provisional concept of psychosis (6) which contains a general structural hypothesis. In short, psychosis is conceived as a splitting of the criterion of reality-testing; in it coexist the normal criterion of reality-testing and one or more of the three pathologic criteria: delusional, hallucinatory and disgregatory. In paranoia, the delusional criterion would be present; in paraphrenia,

the delusional and the hallucinatory; in schizophrenia, the disgregatory and one or both of the other criteria. Although the three types of pathologic assessment of reality have in common the structural characteristic of permitting the affirmation of the existence of a stimulus without sufficient evidence in the opinion of the cultural reference group to which the psychotic belongs, the psychologic material on which the evidence is based is totally different if, for instance, the hallucinatory criterion of reality is compared with the delusional criterion of reality. In the present paper we have not evaluated the quality of the reality judgment of our psychotic group, but have advanced various hypotheses on the structure of the perceptual material used by the psychotic. This material is structurally quite different from that underlying the delusional criterion of reality, and therefore in this paper the perceptual aspects and the paranoid attitude cannot be integrated at the theoretical level. We shall not make any further comments on the paranoid attitude, since its greater intensity in our paraphrenics, revealed through a questionnaire with an auto-reference content, simply provides experimental proof of the existence of said content in psychoses of a paranoid type.

On the contrary, an effort should be made to unify the theoretical interpretations of the results obtained with respect to perceptual factors. If we return to the idea that the hallucinatory criterion of reality-testing is coexistent in psychosis with the normal perceptual criterion, the fact that our psychotic group had a reduced configurational capacity in relation to real perceptual stimuli (Factor A) would permit the emergence of a new and inadequate criterion of perceptual reality. These results are associated with the experimental findings under conditions of sensory deprivation, which show that the deprivation of perceptual patterns is the factor most conducive to the appearance of hallucinations (11).

This tendency in psychotics to produce new perceptual configurations on the basis of evidence which the experimenter cannot accept as sufficient may be observed from the selection of isolated parts of the stimuli in the Street test for the construction of a reply. It may also be observed from the many additional hidden figures of very poor form quality perceived by the patients in the first Factor E test. The results of the three scores for this factor, as well as those for Factor A, suggest a working hypothesis for future research—to the effect that psychotics suffering from hallucinations have a greater capacity for handling two perceptual configurations simultaneously or consecutively, always provided one of the configurations is based on insufficient form evidence as expected of a pathologic criterion of reality-testing, accompanied by a weakened perception of normal configurations (of good form quality).

Moreover, the negative results of the optical illusion tests (Factor B) might be ascribed to the fact that these tests do not bring into play the intensity of the perceptual configuration of normal stimuli, and that there is no opportunity to create additional configurations of a poor perceptual structure. The results obtained by Wachter (12) may be due to the inclusion of manic-depressive patients in his clinical material. According to our concept of psychosis (6), manic-depressive disease should not be regarded as a psychosis from a structural psychologic standpoint. It has, however, been classified as such on the basis of a behavioral point of view or of an analysis of mental content. Both criteria, in our opinion, are obstacles to further progress in the formulation of general structural theories on the psychotic phenomenon.

SUMMARY

The purpose of this study was to make an experimental analysis of one of the components of the psychoticism factor: the hallucinatory-delusional reaction type. An attempt was made to study the structural

psychologic differences between a group of subjects suffering from hallucinatory-delusional psychosis in the active phase and a clinically normal control group.

The two aspects investigated were:

Visual perception (tests taken from Thurstone's *A Factorial Study of Perception*):

Factor A: Intensity of the perceptual configuration.

Factor B: Intensity of the illusory deformation of geometric figures.

Factor E: Manipulation of two configurations simultaneously or successively.

Greater prevalence of the paranoid attitude:

The test battery was given to an experimental group of 20 patients with a diagnosis of paraphrenia, and to a control group of 20 normal persons.

In the perceptual tests the results show that psychotics have a lower perceptual configuration capacity and a greater facility for manipulating two configurations (Factors A and E). But for the latter characteristic to appear, the tests should contain perceptual material from which configurations not foreseen by the examiner may be constructed.

The optical-illusion tests did not reveal any significant differences between psychotics and normal persons. The greater intensity of the paranoid attitude in the psychotic group was confirmed by means of an auto-reference questionnaire.

The results were discussed with a view to their possible integration within a general structural theory of psychosis.

As a working hypothesis for future investigation, the findings suggest that psychotics suffering from hallucinations have a greater capacity for handling two perceptual configurations simultaneously o consecutively (always provided one of thes configurations is based on insufficient for mal evidence), whereas their perception o normal configurations (of good form qual ity) is weakened.

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THE ROLE OF CONTEXTUAL CONSTRAINT IN THE LEARNING OF LANGUAGE SAMPLES IN SCHIZOPHRENIA

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This study was designed to test the hypothesis that the disturbance of language in the schizophrenic stems from a lack of acquisition of the contextual constraints, or associational interdependencies, of language. Specifically, it is postulated that these interdependencies are not so firmly established in schizophrenics as in nonschizophrenics. This would in turn be expected to make language samples of high contextual constraint less redundant, hence more difficult for schizophrenics to learn.

The hypothesis leads to the following prediction:

Increasing the contextual constraint of language samples so that they increasingly approximate the structure of the English language will facilitate learning in nonschizophrenics more than in schizophrenics.

METHOD

Subjects: Four groups of 20 subjects each were studied. Group 1 was made up of acute schizophrenic patients who had recently been admitted to an acute psychiatric treatment hospital. Group 2 consisted of chronic schizophrenics from a State hospital. Group 3 was a non-schizophrenic psychiatric group selected from the same hospital as Group 1. All of the patients in this group had been diagnosed as having affective disorders or psychoneurotic reactions. Group 4 was selected from a group of general medical patients convalescing prior to discharge from the hospital. All patients suspected of being mentally retarded or of having brain damage were excluded.

Language tasks: Miller and Selfridge (1) have devised a method of varying the contextual constraint of language samples experimentally. They constructed their first list by selecting words at random from the Thorndike-Lorge dictionary. Words in this list follow each other completely by chance, and this list is labeled "zero order of approximation" to the English language. List 2 was a random selection of words that occur frequently. List 3 was constructed by presenting a subject with a word and asking him to make a sentence with that word. The first word used by the subject was then used as the next word on the list, and also became the stimulus word for the next subject. This procedure was followed until a list of 20 words had been constructed. For List 4, the subject was presented with two words and asked to make a sentence. The procedure was repeated for subsequent lists by increasing the number of words given to the subject. List 8 was an ordinary text statement. Thus the lists differ as to the degree to which a word on a list depends upon the occurrence of the preceding words, *i.e.*, as to the degree it approximates the structure of the English language in regard to contextual constraint. Eight lists of 20 words from the study by Miller and Selfridge (1) were used in the present study.[3] All lists, including the instructions, had been tape-recorded and were played to the subjects, one at a time, in ascending order of lists, with instructions to recall as many words of each list as possible. A subject's

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[3] A mimeographed copy of the word list will be furnished upon request.

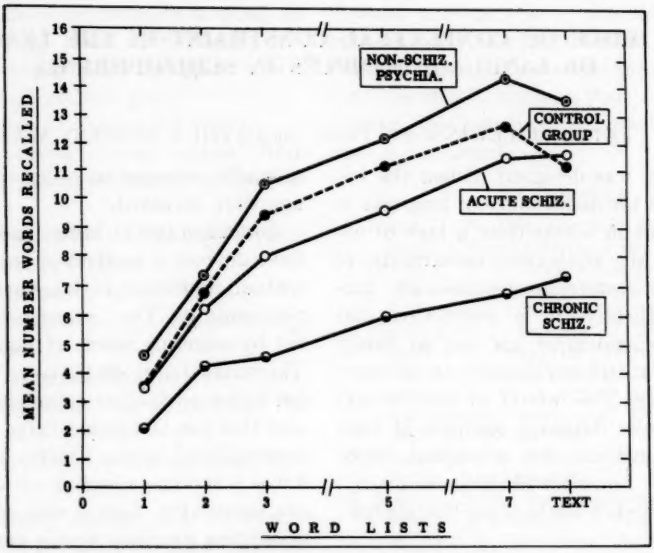

FIG. 1. Scores on the word lists for all groups.

score is the total number of words correctly recalled, regardless of order of presentation.

RESULTS

Figure 1 shows the average number of words recalled on the word lists by the four groups. Lists 4 and 6 were excluded from all calculations because *all* groups showed a relative decrease on these.

The absolute level of performance of the groups on the lists: As can be seen in Figure 1, the performance of the acute schizophrenics was very similar to that of the two control groups (3 and 4) on all lists. An analysis of variance failed to show a significant difference between these groups.

The chronic schizophrenic group, however, consistently scored lower on all lists than did the other groups. An analysis of variance produced an F ratio of 25.8 for this difference, which is significant at the .001 level of confidence.

TABLE 1

Mean Increase in Number of Words Recalled from One List to the Next for All Groups

	Acute Schiz.	Chronic Schiz.	Non-schiz. Psychiat.	Control
Mean	1.79	1.01	1.84	1.52
SD	4.41	2.94	3.21	3.50

Since the groups were not matched on intelligence, and WAIS Vocabulary scores were available for all *S*s, the correlation between the vocabulary test and the total number of words recalled on all lists for all *S*s was computed. The correlation between these two variables was .76. To determine whether the difference between the chronic schizophrenic group and the other groups was a function of the difference between their respective vocabulary scores, 12 chronic schizophrenics were matched on the vocabulary tests with 12 *S*s from Group 4. The t for the difference between the means of the matched groups is .625, which is not significant.

Relative amount of improvement shown by the groups as a function of contextual constraint: For the main test of the hypothesis, we were interested in comparing the relative amount of improvement the

roup showed from one list to the next; ence, difference scores were computed for ll subjects by subtracting the score for ach list from the score on the list that fol- owed it. The average difference between sts for all groups is shown in Table 1. An nalysis of variance of the difference scores howed the difference between groups to e significant at the .01 level of confidence F = 4.4). Analysis of the separate group neans showed this difference to be due to the chronic schizophrenic group. The dif- erence between the acute schizophrenics and the control groups is not significant.

Since the groups had not been matched as to intelligence, the correlation between the difference scores for the word lists and the Vocabulary scores was determined for all groups. These correlations were found to be significant for all groups, and the correlation was especially high for the chronic schizophrenics ($r = .63$). Hence, mean difference scores for the 12 chronic schizophrenics and the 12 control subjects (Group 4) who had been matched for vocabulary were determined; these were found to be 1.17 for the chronic schizophrenics and 1.25 for the controls. The difference between these means is not significant.

DISCUSSION AND CONCLUSIONS

This study was designed to test the hypothesis that schizophrenics do not share in the culturally shared associational expectancies of language, and so will show less improvement as a function of increasing the contextual constraint of language samples.

The results of the acute schizophrenic group were indistinguishable from those of the control group. It is therefore suggested that acute schizophrenics retain the culturally shared associational expectancies of language, and that this factor cannot be responsible for the disturbance in language observed in these patients.

The chronic schizophrenics performed less well than did the other groups with respect to the absolute number of words recalled, the amount of improvement in recall for the lists with increasing contextual constraint, and the Vocabulary test (WAIS). Performance on these three tasks was found to be highly intercorrelated. When chronic schizophrenics were matched with control subjects on the Vocabulary test, the differences between the chronic schizophrenics and the control *Ss* disappeared.

The findings suggest the presence of an overall impairment factor in chronic schizophrenia which extends to all verbal performance measures used in this study.

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BOOK REVIEWS

RAPAPORT, DAVID. *The Structure of Psychoanalytic Theory: A Systematizing Attempt* (Psychological Issues, II, No. 2, Monogr. 6). International Universities Press, New York, 1960. 158 pp., paperbound $3.00

This modest monograph should, I believe, be read and thoroughly discussed as part of every psychiatric residency training program. It is relevant and important.

Such a comprehensive statement of recommendation requires that the reviewer's argument be broadly supported. I intend to support it—upon the grounds of the author's admirable scholarship, and upon his adherence to a reasonable neutrality in presenting the material on which he bases his argument.

The argument is relatively straightforward. It asks the question, "Is psychoanalytic theory worthy of serious consideration by psychologists?," and provides a tentatively positive answer, "Probably, yes."

Before the discussion of Rapaport's thesis, a few words concerning the background setting for the monograph and some notes about the author's scholarship:

Background: The author, whose recent sudden death was felt to be a profound tragedy by behavioral scientists, was one of the nation's most respected scholar-psychologists. He received his Ph.D. in psychology at the University of Budapest in 1938, and had been head of the Department of Psychology, Director of the Research Department at the Menninger Foundation before becoming Research Associate and Senior Staff Member at the Austen Riggs Center, Stockbridge, Massachusetts, in 1948, which position he held until his death. Other writings include *Emotions and Memory, Diagnostic Psychological Testing* and *The Organization and Pathology of Thought.*

The monograph itself is the sixth of a series entitled *Psychological Issues,* edited by George S. Klein. The series is accompanied by a frank editorial statement to the effect that its editors feel that psychoanalysis can claim to be a general psychology, and that they (the editors) have an important commitment to the advancement of psychoanalytic theory. The editorial board consists of well known psychologists and psychoanalysts, most if not all of whom have had affiliation with the Menninger Foundation and/or the Austen Riggs Center.

Scholarship: It is refreshing to follow Rapaport's tracing of Freud's ideas. He is, on the one hand, so well informed concerning the subject matter that he recognizes that the ideas changed over time and had therefore a historical or temporal quality. On the other hand, his purpose in writing the monograph was to survey Freudian ideas with a view to organizing them into theory (or locating a theory already there), and by that token "freezing" them—rendering them into static, stable propositions which could be tested. The monograph is subtitled, "A Systematizing Attempt."

The conflict between the author's scholarship and his theory-building propensities is therefore seen from the beginning of this work. He recognizes the thorny path he has accepted for himself by noting immediately (p. 9): "To my mind it is too early to attempt a systematization of the psychoanalytic theory." Nevertheless, once started, he has no choice but to continue; there is no turning back once an attempt at systematization has been begun.

In addition, Rapaport is clearly familiar with the main currents of psychoanalytic writing in the 70 years which have followed *Studien uber Hysterie.* In tracing the course which psychoanalytic writers have taken to arrive at present-day concepts, he has touched most of the milestones and remained aware of the main trends of psy-

psychoanalytic theory-making. However, it can be predicted that his charting of the course will not please all students, since some rather pointed omissions include the early "character" paper of Alexander, the "treatment-theory" papers of Strachey, and the "super-ego lacunae" papers of Szurek and Johnson.

An example of the author's comprehensive scholarship is this excerpt detailing the development of ego psychology, in which the relevant ideas of authors of significance are smoothly and easily organized into a single paragraph:

> "For a long while the exploration of the ego seemed to be only a contemplated program. Freud expected the information about ego functions to come from the study of 'narcissistic neuroses' (*i.e.*, psychoses), but delayed this study because he considered the exploration of the unconscious to be the primary task. Yet this program was indirectly pursued throughout the history of psychoanalysis in the study of the defenses, censorship, secondary process, and reality relationships. However, Freud did embark (1921, 1923) on an explicit conceptualization of the ego without studying psychoses anew, apparently prompted by the problem of 'the negative therapeutic reaction' and 'the unconscious sense of guilt.' Later he carried the study of the ego further (1926) by re-evaluating the problem of anxiety. Other psychoanalysts followed his lead (Fenichel, 1922–36, 1936–46; Nunberg, 1931; Reich, 1933; Waelder, 1936) and the achievements of this phase of the program were capped by A. Freud's work (1936). The ego-psychological program was then dramatically broadened by Hartmann (1939) and by Erikson (1937, 1945). The ego was explored slowly but so successfully that a broad and still uncharted area was opened up."

The impact of Freud's ideas has been so great in the Western world, and the literary reaction to them so extensive, that to cull relevant writings is no small task. The author has succeeded admirably in a bibliography of 340 items, which is quite adequate and which, except for the few omissions mentioned, the student in training should welcome as one of the best available.

Presentation of Thesis: Other recent writers on psychoanalytic theory, uncomfortably aware of a broad spectrum of criticism, have shown a regrettable tendency to argue defensively. The general theme has been, "If it were not for the enemies and critics of psychoanalysis, I would not have to speak up in our defense." Perhaps such author attitudes represent, in part, a retaliatory responsiveness to certain offensively polemic attacks which have been leveled at their calling from time to time. If such a simple explanation suffices, then the serious psychiatrist deserves better. Just as optimism is not a countermeasure to pessimism, so "being abused" is not an effective countermeasure to abusiveness.

Rapaport, in happy contrast, presents his material with dispassionate simplicity, an economy of words that is nothing short of beauty at times, and a fine clarity of language. For example, the title and text are completely harmonious. He considers the subject of importance to be "psychoanalytic theory," and discusses it as just that. To wit: he has not set out to examine psychoanalysis as a movement, as a group phenomenon, as a quasi-religion, or as an organization. To him, the topic is psychoanalytic theory, and he carefully confines himself to these boundaries throughout.

After having briefly listed background factors about Freud himself which he considers to be relevant, the author comes to grips almost immediately with the central problem:

> "Freud's theory contains four distinct models. They are united in the theory itself, but not in one single model. We will first present each of these, and then attempt to develop a combined model."

The four models extracted by Rapaport

(in contrast to the three as seen by other authors) are labeled:

a. The Reflex-Arc (or Topographic) Model, which he sees as first presented in Chapter Seven of the *Dream Book*.

b. The Entropy (or Economic) Model, which he also traces back to its beginnings in the same chapter.

c. The Darwinian (or Genetic) Model, which theory of psychosexual development he traces to *Three Contributions*.

d. The Jacksonian (or Neural Integration Hierarchy) Model, the source of which he is not so explicitly able to locate, tracing it to such temporally disparate sources as the early work with Breuer in 1893, and the much later paper, "The Unconscious," in 1915. This model, dealing with such concepts as unconcious, conflict, inhibition, unconscious drive forces and counterforces, is the one which has come to be termed "the dynamic point of view," and is the most popular.

Having outlined the disparateness of the various Freudian models, Rapaport then attempts to combine them, using more definitively psychologic language (conation, cognitive) for the process. It is a worthy try, and will no doubt be applauded by theory makers, but let me sum it up in the author's own words:

> "Thus, this combined model ... does unite all the models Freud used. But it does so at the price of falling into six partial models which by their very nature (if not by that of the theory, or even the subject matter) overlap."

Here, one has deep sympathy for the author, who admits in his real and simple humanity that theory-building has its problems, especially when one ends with more complexity than was apparent when one began.

The second main section of the treatise is devoted to a discussion of the structure of the system in terms of systematic independent, intervening and dependent variables. To arrange this, Rapaport reinterprets the sum of psychoanalytic writing in o a series of ten basic propositions. Exampl s of these basic postulates are:

(B) Behavior Is Integrated and Indivis - ble: The Concepts Constructed for Its E - planation Pertain to Different Componen s of Behavior and Not to Different Behavio s (The Gestalt Point of View).

(F) The Ultimate Determinants of All Behavior Are The Drives (The Dynamic Point of View).

(J) All Behavior Is Socially Determined (The Psychosocial Point of View).

Each postulate is presented with care and deliberation, thoroughly considered and examined without contentiousness. When all have been presented, the author examines them as a whole and centers his interest upon a point which, although relatively "quiet," was present in Freud's thinking as early as his association with Breuer, and even today remains crucial.

"1. The psychoanalytic concept of *overdetermination* implies that one or several determiners of a given behavior, which appear to explain it, do not necessarily give its full causal explanation. This is not per se alien to other sciences, though *a principle of overdetermination* did not become necessary in any of them. Psychoanalysis' need for this principle seems to be due partly to the multiplicity of the determiners of human behavior, and partly to the theory's characteristic lack of criteria for the independence and sufficiency of causes. The determiners of behavior in this theory are so defined that they apply to all behavior and thus their empirical referents must be present in any and all behavior. Since there is usually no single determiner which constantly assumes the dominant role in a given behavior, other determiners can hardly be neglected while a dominant determiner is explored. When favorable conditions make one determiner dominant, the investigator is tempted to conclude that he has confirmed a predicte l

unctional relationship—as he indeed as. Regrettably, the attempt to repeat he observation or experiment in question often fails, because in the replication either the same behavior appears even though a different determiner has become dominant, or a different behavior appears even though the same determiner has remained dominant."

Still speaking as a psychologist, the author next presents the problem of quantification in testing psychoanalytic propositions, and again finds the great obstacle to be the "looseness of the theory." Describing the present status of the system, he states:

> "The theory of psychoanalysis grew by successive spurts in the 50 years of Freud's work. Additions and revisions make it appear more like a patchwork than an architectonic design, since their consequences for the structure of the system have often remained a matter of a passing comment by Freud or isolated papers by other psychoanalysts. This in itself bespeaks a looseness of the theory and the lack of an explicit canon according to which revisions and additions are to be fitted into its system. Yet psychoanalytic theory does have an impressive structural unity, though it is hidden under the layers of progressive additions and modifications, and has not been disentangled and independently stated."

For completion, he concludes short notes about evidence for the system, ranges of application, and the achievements of the theory, provides some words of advice about methodology, and has this to say about the future:

> "The future . . . may hinge on the solution of the problem of structure formation and learning. If that solution should arise from the matrix of psychoanalytic theory, the latter may become the core of psychology proper. If the solution should prove relatively independent of psychoanalysis, then psychoanalysis is likely to become a relatively subordinate part of the general theory of psychology as the core of its clinical and motivational theories, but its concepts and theories will be reducible to more fundamental ones. The existing learning theories have not accomplished this reduction and it seems unlikely that they ever will."

Finally, in a section which will no doubt be widely discussed among psychologists, Rapaport slaps mightily at scientific method when used as canon:

> "The stress on the 'scientific method' becomes an obstacle to theoretical progress in several ways. *First,* the stress on teaching the scientific method and the design of experiment diverts attention from training in observation. *Second,* it discourages the budding investigator's interest and trust in his own hunches and speculations. *Third,* it makes the 'scientific method' and the 'design of experiment' appear as a sure-fire way to produce 'research findings.' The findings thus produced clutter our literature and crowd out the interest in *methods* of experimenting and observing. *Fourth,* it leads to a publication policy (and, through it, to a training by precept) such that the publications conform to the 'scientific method' and cover up the actual tracks of the investigator even when by chance his tracks would be worth knowing. The publications read as if investigation consists of nothing but the application of the scientific method. Thus to the novice, our (and what is more important, his own) actual disorderly ways of productive thinking must appear as inadequate and falling far short of this forever unattainable ideal. His self-observations, which show him that his thinking does not follow the 'scientific method,' become the sources of a gnawing self-doubt, which in turn only too often leads to a sterilizing constriction of thought: discipline, the very lifeblood of scientific thinking, be-

comes a bane. No wonder that in our literature few authors are surprised, few things are surprising, and a deadly boredom prevails, aided and abetted by what the given journal considers to be *the* form of scientific reporting."

Critique: In that I have so long been a student of Mencken and feel, with him, a certain obligation on the part of the critic to speak out when moved, these observations seem relevant:

Systematizing, theory building, without specific purposes in mind, may well be missing the point, as Freud himself suggested when he discussed the over-evaluation of objects and ideas. Let me illustrate: Sigmund Freud was a wise man, wiser than most. When he thought clearly, he thought with unusual clarity and discernment. This does not, and cannot, mean that Sigmund Freud thought (or wrote) clearly at all times, was wholly and completely consistent in all his ideas, or was, even when inscrutable, merely disguising some ultimate truth.

To wit: Sigmund Freud was a human being and one who had a great many ideas, plus an extraordinary flexibility and freedom in expressing them. He could not, except during his most muddled moments, have expected that all of the accumulated ideas he expressed in a lifetime could be placed together into a holistic, overall-cohesive theory.

It has been obvious for a long time that there is no psychoanalytic theory *per se.* There are, rather, a series of important and significant psychoanalytic *ideas.*

I do not see the necessity for Freud's ideas to be held sacred and to be preserved only on that account, or to be found overall-cohesive to be useful. Psychiatry and psychology have more important concerns, and Rapaport, with his meticulous examination, has demonstrated this point, whether he set out to do so or not.

He deserves to be read. Highly recommended.

Kenneth L. Artiss

DOLLARD, JOHN AND AULD, FRANK, JR. *Scoring Human Motives. A Manual.* Yale University Press, New Haven, 1959. 452 pp. $9.50

LENNARD, HENRY L. AND BERNSTEIN, ARNOLD (WITH HENDIN, HELEN C. AND PALMORE, ERDMAN B.). *The Anatomy of Psychotherapy.* Columbia University Press, New York, 1960. xviii + 209 pp. $6.00

John Dollard has said that those who seek out psychologists in their assigned stations in hell will find them busily engaged in content analysis. Since the process itself is so laborious, one may well wonder why certain behavioral scientists inflict it upon themselves. While the two books here reviewed do not portray the task of content analysis as an experience of hedonistic delight, they do make a strong case for its importance in the study of psychotherapy. In this respect there is a rather attenuated similarity between them. They are also alike in their dedication to a clearly formulated theoretical frame of reference. But here the similarity ends. In their theoretical approach, Dollard and Auld are committed to a sometimes strained marriage between psychoanalytic and learning theory, whereas Lennard, Bernstein, Palmore and Hendin are committed to the use of sociologic concepts regarding small group interaction and communication in the study of the psychotherapeutic dyad. Dollard and Auld approach their task as "insiders" who are systematically surveying terrain previously viewed impressionistically; Lennard *et al.* approach theirs as "outsiders" importing "alien" instruments for mapping the same terrain. Dollard and Auld designate their work as a manual, although it is clear at a glance that their book is in many

ways much more than a manual; Lennard and his colleagues offer their book as a report of research accomplished, although some of their sections on methodology read like parts of a manual. Anyone who has found occasion to contend with the complex and shifting problems that arise in the content analytic study of psychotherapy will find both books helpful and, in different ways, encouraging.

Many sections of the Dollard and Auld book read like chapters in a basic introduction to content analysis. They propose the following definition: "Content Analysis, in its general social-science definition, means the classification of what is said according to some set of categories. As we understand the term, content analysis does not imply any particular kind of categories. Rather, the set of categories can be chosen so as to be appropriate for the problem being studied" (p. 27). Both units into which utterances are divided and the categories into which the units are placed constitute the functional core of a content analysis system. Units and categories derive from the conceptual framework within which one operates and from the kinds of problems one undertakes to investigate. In a scholarly review of previous content analysis studies of the therapeutic interview, written by James E. Dittes and included in one of the appendices, two basic types of categories are delineated. One is based on such easily defined objective categories as the structural properties of language, variability in words used by participants, pauses, and the specificity of verbal communications. In this type of system, the degree of inference in the assignment of a unit of verbalization to a category is minimal, resulting in a high level of reliability. However, with such narrow-gauge objective systems, serious difficulty is generally encountered in attempting to establish a relationship between content categories and clinically meaningful aspects of interview behavior. The second type of system described in the Dittes summary is exemplified by the work of Dollard and Auld. They assume that "single partial measures" cannot adequately portray the complexity of interview behavior, and therefore are poor vehicles for testing out important hypotheses. In Dittes' words, "... one must examine the full scope of what is said, in its full context, to understand the transaction" (p. 329). Since the scoring of comprehensive categories calls for the exercise of a high level of inference, reliability can be achieved only through careful definition. Hence, the authors produced a manual which is a significant contribution to the definition of many constructs in Freudian and learning theory. In this "manual" one finds a nice balance between the clarification of theory and definition on the one hand, and the empirical application of defined categories to the study of interview behavior on the other.

The welding together of two theoretical systems such as Freud's and Hull's into a single operational scheme calls for a complementalness that is not present at all points of contact. However, in spite of the difficulties associated with the attempted reconciliation of the two theoretical systems, a careful reading of the coding scheme is an immensely rewarding experience. It represents a major effort to systematize concepts frequently used with discouraging ambiguity by clinicians. A glance at the coding categories of patient behavior indicates that more than motivation in its usual sense is encompassed. In addition to such motivational categories as anxiety, dependency, hostility, love, social mobility and sex, there are also symptomatic response categories and others dealing with the immediate doctor-patient relationship. With reference to the coding system, Dollard and Auld emphasize repeatedly that a scoring category was never proposed simply because it was relevant to their theoretical ref-

erence systems. Theoretical considerations were balanced by the empirical pull of the interviews they studied and the problems they wished to explore. If a particular interview seemed to call for a category, they would define it and, as they put it, "place it on probation." It survived its probationary period only if it proved to be useful across a wide range of interviews. Thus their signs have the merit of proximity to the interview material, but are of course limited by the interviews used.

The courage of Dollard and Auld in extending their coding scheme into complex and highly inferential areas is exemplified by their formulation of criteria for the scoring of unconscious motives. In this section the reader will find an exciting example of the creative use of learning theory principles in the clarification of a construct that psychotherapists have long used but done little toward delineating in specific, clinically operational terms. Consider, for example, the particular definition that Dollard and Auld give the "conscious" versus the "unconscious." The former is regarded as that which can be named or labeled; the latter, as that which cannot. Lest such a behavioristic distinction appear too limited or alien to the clinician, the authors add that the crucial test is not that of simple neglect to name, but rather inability to name. The discussion of repression and the unconscious is then followed by specific, "manual"-type instructions for recognizing and scoring repression in verbal material.

In overall balance, such issues as unitizing, reliability and validity of categories used appear to be dispatched with relative brevity. In actuality, however, they are not neglected; they are simply dwarfed by the completeness and the elegance with which the authors developed their theoretical position. It is characteristic that instructions for unitizing are not simply presented in a work-sheet manner, but follow a comprehensive discussion of different types of units, and the relationship between unit size and problem under investigation. Since Dollard and Auld were interested in studying the detailed process of psychotherapy, they selected a fairly microscopic unit, the independent clause.

Similarly, reliability data are presented after a general consideration of problems in the assessment of reliability in content analysis. In defense of their publication of a book which is long on reliability data and short on validity data, the authors have this to say: "It should be apparent therefore that developing reliable measures of the events in psychotherapy is no pious and empty exercise, nor is it a compulsive absorption in minutiae. It is, instead, a necessary step in the development of a science of psychotherapy.... Much of our work ... can best be described as the devising of reliable measures for concepts such as resistance, conflict, anxiety, and hostility which have hitherto been poorly defined" (p. 295).

Evidence as to the yield in terms of validity is, by the authors' own admission, as yet skimpy. Some data are given, based on the scoring of 68 interviews conducted by six different therapists with eight patients and one clinically normal subject. They found that the system was flexible enough to be applied to the different therapist-patient dyads. Although the results reported are not extensive, they are suggestive of the kinds of problems the system is equipped to cope with. These include the mutually exclusive occurrence of certain motives, the interaction of motivational expression and resistance, and the contingency between certain types of therapist behavior and patient response. The studies completed so far are congruent with previous ones conducted by Auld and White, and by Murray, and point broadly to the course that future investigation may follow.

The focus of Lennard, Bernstein, Palmore and Hendin at the Bureau of Applied Social Research, Columbia University, is radically different from that found in the

work of Dollard and Auld. One need only scan their final chapter (entitled "Some Last Thoughts") for an impression of the contrast. Lennard and his group emphasize that one of their objectives is to demonstrate how the concepts of one discipline (sociology) can contribute to the understanding of problems faced by another discipline (psychiatry). Having thus written their own credentials, they advance a number of postulates about the psychotherapeutic interaction, derived from Bales's small-group theory, and apparently supported by the data they present. They eschew the intrapsychic sphere, the interplay of drive and defense, and turn their attention to the interaction between the two individuals that constitute the psychotherapeutic dyad. Their postulates are explanatory of this interaction as though it were occurring in a small group, reduced to its minimal structure. They utilize the therapeutic situation as an opportune one for studying a very special type of small-group verbal behavior, making explicit their lack of interest in such therapeutic problems as strategy, progress or cure.

The authors take issue with an assumption, held by many in psychiatry, that the differences between therapists in terms of theoretical bent are significant in determining therapeutic practice: "We believe that the most important contribution of therapy lies in the experience, in the total and recurrent pattern of patient and therapist interaction, extending over an enduring period. The enormous amount of similarity in therapeutic systems suggests to us that what is shared by different therapist-patient pairs may be at least as therapeutic as that which is unique" (p. 194). With this type of system-sensitive approach the authors then find "... a good deal of evidence that therapists and patients interact in the true sense of the term, and that feedback plays an essential role in treatment" (p. 194). Lennard *et al.* would agree with Dollard and Auld that at the behavior of both therapist and patient can be understood only when one postulates the active influence of determinants that are partially out of the awareness of both participants. But Lennard and his group populate the area of unawareness, not with drives and conflicts, but with small-group system laws and pressures. They argue that the training of therapists should concern itself with the fostering of system awareness in the practitioner, so that he might better adjust his therapy behavior to that of the patient and to the lawful requirements of the system. In brief, Lennard *et al.* are interested in the small-group structure within which verbal communication takes place between patient and therapist.

This structure is determined by various laws which govern verbal communication. Thus, they advance certain hypotheses pertaining to the therapeutic dyad as an informational exchange system. They postulate a systematic relationship between "the amount and quality of information put into the system by the therapist" and "the amount and quality of the informational output of the patient." Moreover, they regard this relationship as reciprocal. They found that therapist statements containing propositions of low informational specificity consistently yielded a greater number of patient propositions than did therapist statements of high informational specificity. Reciprocally, as patient output fell, therapists tended to increase the specificity of their verbalizations. Although it is not possible to review the various hypotheses and the extensive findings of this study here, all who are interested in this novel view of the psychotherapy process are assured that they will find a reading of the book most profitable.

The data consist of recordings of eight therapies (four therapists with two patients each) over a period of eight months. There are 500 recorded sessions, 120 of which were subjected to intensive content analysis. Though the four therapists consider themselves to be psychoanalytic in orientation,

Lennard *et al.* assure us that they differ considerably from each other. This point is stressed presumably to emphasize the wide-range applicability of the relationships discovered. Yet the authors are hesitant on this point. They regard their study as a pioneering effort in the development of a methodology and the formulation of hypotheses, not as a venture in fact finding.

It is readily apparent that methodologically their approach is quite different from that of Dollard and Auld. Unlike the latter, Lennard *et al.* do not limit themselves to content analysis. Some of their data were obtained through the use of questionnaires and structured interviews. Dollard and Auld meticulously built their coding system into a complex and plastic instrument for exploring a large number of relationships. Lennard and his group placed much more emphasis on their theoretical system than on any one of their many coding schemes. In fact, implicit in their report is an attitude of casualness to both their instruments and their statistical treatment of results. No reliability data are given. Statistical tests of significance are frequently quoted in footnotes to tables; often they are lacking. Even in a hypothesis-hunting sort of venture there would appear to be no justification for this type of inconsistency. In the end, one is left with the impression that Lennard and his colleagues have developed a powerful conceptual scheme for the study of the interactional aspects of psychotherapy, but have not as yet demonstrated its validity in statistical terms.

Psychologists have now been in the business of content analysis of psychotherapy for well over a decade. Some of the newer approaches were displayed and worked over at the American Psychological Association conference on research in psychotherapy, reported by Rubinstein and Parloff in 1959. Few of the approaches discussed at this conference have demonstrated the vigor of conceptualization found in the two schemes discussed in this review. The optimism that one tends to feel about these two studies is diminished only by the long history of attrition in such schemes when used by researchers other than the originators. This reviewer is nevertheless convinced that the two are both powerful and complementary methods for the study of psychotherapy. Both augur well for the research that will undoubtedly follow.

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